

OBITUARY NOTICE

KENNETH J. W. CRAIK, 1914-1945

Between the two wars a small group of British psychologists used to meet twice annually to discuss problems in which they were interested. On one of these occasions I was walking along a country road with Prof. James Drever. He said, "Next term I am going to send you a genius". That was the first I heard of Kenneth Craik. It was a sufficiently startling introduction, and all the more so because I had long ago learned to have the deepest respect for Prof. Drever's judgement about his students.

I then heard a little more about Craik's life, achievements and promise, and it was with the liveliest anticipation that I looked forward to seeing him in October 1936. Vividly I remember that first meeting. He came into my room at the Laboratory and my immediate impression was of a tall, rather powerful, spare frame; a face pale but full of life; a firm chin, straight mouth, singularly attractive dark eyes, and above a shock of black hair. From the beginning he was wholly 'at home', as we say, with any amount of genuine modesty, but not a scrap of false humility. He knew, and within a very few minutes I knew, of the power that was within him. We talked of what he had done, and more of what he would do. He was not then certain of what he would do in detail, for it was one of Craik's outstanding characteristics that there was very little which lay in the line of developing knowledge in which he was not profoundly interested.

At the Edinburgh Academy he had been a classic, and at the University of Edinburgh he had, with great distinction, studied philosophy under Kemp Smith. Prof. James Drever has the honour—and a very great honour it is—of being the first to attract and hold him to the systematic study of psychology. But I am sure that all those who took part in his training would agree with me that a great amount of the best preparation for the work he was to do came from a most happy and free home life, which encouraged him to explore every path of opening interest, presented to him the widest possible outlook, and fostered a love of honest thinking. Particularly his holidays with his parents, by motor boat into remote parts of Scotland by sea and river, gave him independence, a capacity to deal with sudden emergencies, both small and large, and much knowledge of the natural life of flowers and beasts, and of the structure of the earth.

When he came to Cambridge, Kenneth was absolutely certain that psychology was the subject which above all he desired to study, but he was still a little undecided: should he take for his field the higher mental functions, or should he elect to follow the traditional approach through research upon the special senses? He did not hesitate for long—that was his way always when the question was one which concerned a course of action affecting mainly himself. Already, for Prof. Drever, he had begun some studies of brightness discrimination and dark adaptation. At Cambridge at that time was a very active group of research students and others at work upon sensorial problems, particularly of hearing. There was also—and this may easily have been the deciding factor—an

exceedingly attractive workshop which captured his imagination from the moment at which he entered it. He decided to research on visual problems, and particularly on visual adaptation and after-images.

Thus began three crowded and happy years. There was, I think, nothing in a very busy and contented Department which Kenneth failed to influence and to help. The very first time I met him, out from his waistcoat pocket came his famous working model of an internal combustion engine. Everybody who had any apparatus problems quickly learned to go to Craik for help, and I cannot remember any case of a vain appeal. He worked with extraordinary rapidity, both in devising and in making instruments and gadgets of all kinds.

But, sure of himself as he was, he never, as some people do, thought of himself as beyond the stage of learning. He went to all the courses he could. I think of him as one of the most stimulating students in my own Discussion Classes that I have had the great fortune to know. He broke out of the conventional limits of psychological training. He went to the Cambridge Technical School for a course on plumbing and welding. He did a lot of physiology, and later on, brain anatomy.

His activity was by no means confined to his own immediate topics. He worked with Oliver Zangwill on some problems of Gestalt Psychology. He was ready to be an observer in every experiment. At the Psychological Society he would come in, often a bit late, sit cross-legged on the floor, listen, and then start some lively discussion, sticking to his point with persistence and good humour, and with his wonderful enjoyment of his own jokes, some of which were very good.

In 1940 he obtained the Cambridge Ph.D., and then, a year later, having enlarged his dissertation considerably, he became, at the first attempt, a Fellow of St John's College, which he had joined when he came to Cambridge. The Thesis was entitled, "An Experimental Study of Visual Adaptation, and a Discussion of some more general Psychological Problems". The title was characteristic. There are some people who take a specific problem, answer it, and pass on to something else. Their work may be very good, but it lacks true fruitfulness. Kenneth was not in that class. I do not think he ever did an experiment, however simple and small it may have appeared, which was not informed by some idea which took its issues at once into a wide field of principle. This thesis, which has not yet been published, although it is greatly to be hoped that publication will come before long, is strongly marked by what became one of the leading ideas of all his work, a lively recognition of the interrelation of physical, physiological and psychological problems and issues. Already he was searching for and using physical analogies: "The eye resembles a multi-range meter. . . . When set to any one range, or adapted to any one illumination, it is sensitive to rapid variations in illumination over a certain range. If it is adapted to a different illumination its whole range of sensitivity is shifted bodily to the new adapting illumination. This 'range-setting' is automatic, since the change in adapting illumination itself occasions it. It accounts for the great precision of the eye in distinguishing small, rapid changes in illumination and its inability to detect slow ones, or to act as an absolute photometer." At the same time he devised, carried out and discussed a number of new experiments dealing with the transmission and scattering of light by the eye media and with peripheral photochemical and neural processes. Further, he was able to combine all his results into an illuminating general study of adaptation from a psychological point of view proper and of its biological significance.

The dissertation was not fully completed when the war broke out. Kenneth came to me at once to discuss whether he should join one of the Fighting Services, which, for

many reasons, he genuinely desired to do. The answer was plain, as he well knew. His very unusual equipment should be put at the service of any authority which needed it, naval, military, air or civilian. For many would need it; and in a very short space of time many did. From 1939 to 1945 it is the simple truth to say that he was a key man in the scientific service of the country.

From the beginning I decided that he should have the fullest independence possible. He would have had it in any case; that I was glad and proud for him to have it made no difference, and never, during these incredibly busy years, did he once fail the trust we had in him, or once take any undue advantage of it. I am sorry, and even a little ashamed, that when I first sent him away, entirely by himself, to discuss some technical psychological problems about the role of the human operator in the manipulation of certain instruments of war, I wondered a little what sort of a show he would make. I need have had no misgiving and ought to have had none. In these years he was to meet and almost immediately to win the confidence of all sorts and conditions of men, from scientists of international repute in very many different subjects to laboratory assistants and working mechanics; from Admirals, Generals and Air Marshals to ratings, privates and airmen; from leaders in industry to the rank and file of the workers. When he made any definite pronouncement everybody was sure at once that he knew what he was talking about. When he did not know, he never pretended, but was eager and astonishingly quick to learn. And when work was over for a time he was a thoroughly good companion.

The story of how he worked, long, intense, absorbed days and nights; and of what he did, solving all manner of mechanical and electrical problems in his stride and getting at once to the point of his problems in simple but brilliant experiment, cannot here be at all fully told. He was a very active member of the Vision Committee of the Flying Personnel Research Committee; he belonged to the Military Personnel Research Committee, and some of his remarkable work concerned tank equipment; he was an original member and became the Chairman of the Target Tracking Panel of the Ministry of Supply, and was also on its Servo-mechanisms Panel. He wrote many reports, all concise and to the point, and almost all of them containing descriptions of highly original methods and results. Although these papers could not be published, they were widely known and sought after. Many of them carried further the work on vision, dealing with dark adaptation, the use of night photometers, and definitely establishing several of the basic principles in radar display. Others broke new ground in studies of the principles of bodily mechanics, and the physiology and psychology underlying the efficient combination of motor responses in the control of instruments.

One story I should like to record. Kenneth and I had been out to look at some new anti-aircraft equipment. We were being driven back to Cambridge in a light car, by a very accomplished Services chauffeur at what may fairly be called a 'tidy pace'. The roads were greasy. For some time I had been trying to think how the conventional laboratory procedure for the study of fatigue might be supplemented in certain ways, perhaps improved. The common methods, based upon an investigation of simple and relatively isolated muscular and mental processes, seemed to me so devised that practically only three types of result could be recorded accurately: the amount of deterioration of work, checks and spurts in work, and the final collapse of work. I thought something was needed which would show clearly and exactly how skill, long continued, may change and perhaps disintegrate. So I asked Kenneth whether perhaps it would be possible to design an experimental cockpit, so that the essential control responses of the aircraft-pilot, flying

on instruments, could be accurately recorded, if necessary for long periods, and we should know, not only whether less or more work was being done, but also by what changes in the co-ordinated activities these, and other variations, were brought about. He jumped to the idea. He pulled out his wonderful black wallet, stuffed to overflowing with odds and ends of bits of paper, with their jotted notes in a strange handwriting about projected experiments, with dates of engagements seemingly in a terrific muddle, with impromptu drawings of apparatus. He found an available bit of blank space. A diagram began to grow.

We were driving down a steep hill. A car immediately in front suddenly stopped. So did we. We got into a terrific skid, made a right-angled turn and, by a bit of fine emergency control by our driver, came to a full stop with the front wheels of the car cocked high up on a steep bank a few inches from a thick-set hedge and in the hedge a number of uncomfortable looking tree trunks. Kenneth straightened himself up from the side of the car, rubbing a bruised arm. He was grinning happily. This was just what he enjoyed. He went straight on with the job.

The very next day he was in the Laboratory workshop fashioning the experimental controls for the first Cambridge cockpit. The design was his. He and George Drew together did the work. The whole thing was a very brilliant and beautiful application of calculating machine principle to a complex psychological problem. It was built in our own workshop, with slender resources and at trifling cost. It was to stand up to years of hard work, and first by the very expert research of Drew, and later by Dr D. Russell Davis, to open up what may well be a new chapter in experimental psychological development. For not only did it show that 'skill fatigue' is in many ways different from that deterioration which long spells of work may impose upon simple muscular and mental tasks, but also it demonstrated that it is possible to submit highly complex bodily and mental processes to exact and illuminating measurement.

Craik's work was rendered possible by constant and generous support from many different quarters. But above all it was Sir Edward Mellanby and the Medical Research Council who had encouraged it, aided it without stint, and given it the widest opportunities. In 1944 the Medical Research Council agreed, should the University concur, to establish in the Cambridge Psychological Department a unit for Research in Applied Psychology. The offer was accepted and the unit established forthwith. The idea, the name, and above all the appointment of Kenneth Craik as the first Director of the unit, were all due to Sir Edward Mellanby, to whom psychology in Cambridge already owed more than can be expressed. In spite of all his many other preoccupations Craik found the time to inspire every branch of the work of the unit in the most practical manner, showing an unusual power of leadership.

On 7 May 1945 Kenneth came into my room about 6.30 p.m. to tell me about his movements for the next few days, and to discuss plans. He was full of all the usual enthusiasm. We were to meet at St John's College a little later, for it was the anniversary of St John the Evangelist and the annual dinner, greatly abbreviated because of war conditions, was still held. Soon after the dinner began news came that Kenneth had had an accident. I left the College. It was the eve of V.E. day. Flags were flying, many people pursuing their cheerful plans. Back went my mind like a flash to another public holiday years ago when I had come out of the same College gates into a world at play, knowing that W. H. R. Rivers, whom I had seen not long before full of vigour and plans, was lying dead. I went slowly to the hospital, and learned for certain what it seemed that for

certain I knew already. There was nothing at all to be done. Kenneth was unconscious. He could not recover. Consciousness never came back to him, and that was a good thing; for if it had it would have meant pain and distress: these, at least, he escaped.

For the last six years—and they are the only ones I can write of with first-hand knowledge—Craik's life in Cambridge had been a very happy one. I often find myself wondering to what his power and its achievements were due. Partly they were due to an exceptionally acute and quick mind; partly to a controlled but agile imagination which was able to take practical problems, solve them, and at the same time use them in the interests of basic research; partly to a kind of training which gave him wide interests and a capacity for honest thinking, but did not prematurely tie him up in any specialism; partly, paradoxically, to a body that was not naturally very biddable, but which he so far trained that he became a most beautiful craftsman and no mean player of certain quick ball games; partly to a temperament which was appreciative before it was critical, so far as other people were concerned, so that he gave his best to them without reserve and got their best out of them without effort. Kenneth himself constantly acknowledged the fact that much of his success was due to his good fortune in having the most loyal experimental assistance, first from Mrs S. J. Macpherson and then from Miss M. A. Vince, and to his coming into a small group of people all intensely interested in the development of psychological science, and both critical, and co-operative to an unusual degree. Behind and deeper than all these, and the many other similar influences which made him what he was, there were other things which maybe fewer people had the chance to know.

Happy these years were beyond question; but they had also their difficulties and indecisions, sometimes long and hard to resolve. He took tremendous physical risks, not always necessary ones either, and genuinely enjoyed them. More ordinary things could worry him. He had frequent long journeys, with much discomfort, little chance of food, and social encounters impossible to foresee and nothing whatever to do with the purpose of the journey, which were at times irksome to him. When the journeys were over he would joke about them, but their anticipation sometimes bothered him considerably and put him off his work. Making decisions about personal priorities which could perhaps seriously affect the careers of others he found very difficult, as perhaps most people do who have a wide tolerance for many different ways of thinking and for many different qualities of men. More important still, probably, were certain intellectual conflicts, never wholly settled, some of them with emotional roots deep in his earlier years.

One of these sprang from his outstanding capacity for designing and making instruments of all kinds. He found it hard to refuse any problem which gave him a chance to invent some new piece of apparatus, especially of the mechanical kind. There was a danger in this. He knew that it was to some degree interfering with his interests in basic research. Some of his later work, under the war stress, became in fact a little less rigorous than the earlier and a little less scientifically satisfying. There was a chance that he might find himself caught up in one long swirl of merely *ad hoc* investigations. But all those who knew him best were quite sure that this would not have been a lasting danger.

There was a more stubborn conflict than this one. Like many other people who have achieved striking originality Kenneth was exceedingly suggestible. Receptivity to ideas of all kinds and from all sources may well be one of the most effective conditions in the production of that sort of intellectual ferment out of which new developments arise. In Craik's earlier life most of the strongest influences came from people with profound artistic and humane interests. He gained and kept a love of beauty, particularly in poetry

and in music, and treated the human mind as something unique, not capable of adequate expression in terms applicable to anything but itself. Then he came to an environment in which the strongest intellectual stimuli were scientific. Many of the people he knew well and most deeply respected were trying to find mathematical expressions for a large variety of relations between stimuli and the responses which they set up in animals and human beings. The long-standing psychological controversy about the quantitative nature of sensations fascinated him, and although his own studies in sensory adaptation convinced him that none of the earlier formulae proposed for the expression of this particular relationship between the gradation of stimuli and an accompanying gradation of response was adequate, he hoped and believed that more satisfactory formulae would be found. It may even be the case that his rather sudden switch over from the humanities to natural science led him to exaggerate the differences between the two points of view.

When he was seventeen, Kenneth wrote an essay on Immortality.* It is intended to demonstrate, or at least to declare, that there can be no satisfactory accounting for man's life and mind as if they were machines. The notion that keeps cropping up is that however anything may be constructed its beauty may well be something different from the manner and material of its making. "And it is comforting to think that we need not despise the beauty of things because they consist only of electrical energy; but rather admire them whether they do or not."

The only completed study of any length which he published was his small book on *The Nature of Explanation* (Cambridge University Press, 1943). In this he appeared at first sight to be taking up a position diametrically opposed to that of his early essay. He argued that perhaps the human mind and body operate exactly according to the mechanical principles of the complex calculating machine or certain developing forms of servo-mechanisms, regulating its output not only according to the quantity but also according to qualitative features of the input supplied by its appropriate stimuli. Others, watching these machines at work have seen them as illustrations of how the human mind, faced with special problems, mostly of a practical character, has set to work to devise instruments which tackle them as nearly as possible with the results that the mind itself might achieve. He seemed to be trying to see them as evidence that in so far as they are successful, they show how the mind works, not in inventing the machines and using them, but in actually solving the problems. If therefore the flexibility of such machines could be so enormously increased that they could deal with as many and as varied situations as the mind and body can master, this would prove that they work just as the mind and body do, and further, the principles explaining their operation would be exactly those principles which are used in the current explanation of the operations of any system called mechanical.

Both these inferences are dubious. Both seem a far cry indeed from the earlier essay which asserted that "the mind is . . . a fit instrument for any research, and something to be treated with a reverence that we can never feel for what may turn out to be a novel and complicated kind of engine".

Many a time in the last year or so I went to his room in the Laboratory and found him, with absorbed delight, experimenting with lovely self-regulating machines, adapting them to pick out and respond to stimuli in rapid sequence in a different manner from their reaction to stimuli in slower succession, and even to segregate and do something special about changes of shape, like kinks in a straight line. When they broke down, as they not

* I am deeply indebted to his mother, Mrs M. Sylvia Craik, for a copy of this essay.

infrequently did, he would look up with a rueful smile and go on trying. In those smiles I think that perhaps I was not wrong in finding some quiet but deep satisfaction. He was so honest that he had to go right through with the mechanistic hypothesis, pushing it to the uttermost. Yet with him, as with many another, there remained the conviction that man's life and search are a perpetual adventure, and that all our advances towards self-knowledge are promises without end.

F. C. BARTLETT

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Abbreviations

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B.P.C. = Military Personnel Research Committee.
M.R.C. = Medical Research Council.

A.O.R.G. = Army Operational Research Group.
A.P.U. = Applied Psychology Unit.
R.N.P. = Royal Naval Personnel Committee.

A.R.L. = Admiralty Research Laboratory.

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THEORY OF GENERAL FACTOR¹

By C. SPEARMAN

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A. EXISTENCE OF 'g'

I. PROBLEM

(1) *Metaphysics*. In the present article we are concerned with the reputed fact that cognitive ability discloses to observation a general factor, especially the one which has been designated as 'g'. And for convenience we will treat two main questions very distinctly. Does such a factor really exist? If so, what is its nature? Taking the former question first, let us clear the ground by repudiating all the metaphysical implications that some authors have sought to read into the terms *really 'exist'*. Such topics belong, it is believed by the present writer, not so much to empirical as rather to philosophical psychology, with which we need not here trouble ourselves. (We have treated it freely elsewhere, as in *Psychology Down the Ages*(¹)). For us, the question as to the observation of 'g' reduces itself to that of validity of certain 'hierarchic' equations. We have only to inquire whether or not these have been sufficiently well satisfied for the purpose of determining and stabilizing the factors at issue.

(2) *Vicissitudes of research*. Now at first sight this inquiry would seem to have suffered a notable set-back. For in the earliest review of the evidence (quoted in (2), chap. x) a complete list of the twenty researches then available has shown *all* of these to have satisfied the said equations. But then the wind shifted. More and more frequently cases were reported of the equations failing. Louder and louder grew the voices of criticism.

But to these again came vigorous replies. The change of results was attributed to change of attitude. For whereas the earlier workers had expressly endeavoured to find conditions that would satisfy the equations, the later efforts had aimed rather at finding those which fall short of doing so. Science, obviously, needs information of *both* sorts.

¹ The second part of this paper (B) was read by Dr LL. Wynn Jones at the Glasgow Meeting of the British Psychological Society on 31 March, 1944, and he has incorporated minor changes in the article as advised by the author.

(3) *Conditions for proof of existence.* Let us first then begin by indicating what conditions are really essential. First of all, as already mentioned, the correlations at issue must display a marked tendency to 'hierarchy': they must, that is to say, tend beyond the range of mere chance, or error, to satisfy the following equations:

$$\frac{r_{ap}}{r_{aq}} = \frac{r_{bp}}{r_{bq}} \text{ and therefore also } r_{ap} r_{bq} - r_{bp} r_{aq} = 0.$$

When, but only when, these equations are fulfilled, then all the test-scores or other variables can be reduced to terms of a single one.

As next condition—and one much more likely to be overlooked—every tetrad stands on its own basis. The hierarchy need not be displayed by *all* the tests at issue, but only by a minimum of four of them. This is expressed in the following equation:

$$r_{ag}^2 = r_{xy} r_{xz} / r_{yz}.$$

Here the triplet of given correlations on the right serve conjointly as 'reference values' or 'jumping-off ground' for the 'g'-coefficient on the left. Any further given triplets can only supply corroboration or at any rate correction.

As yet another condition of the factorization, we must bear in mind that even the most careful experiments are liable to suffer from various disturbances that require amendment. In such case, the required corroboration is afforded, not at all by the raw values, but wholly by the amended ones. Here come pre-eminently the disturbances that arise from sampling and whose magnitude is indicated by the 'probable error'.

II. CRUCIAL EXPERIMENTS

(1) *Contribution of Stephenson.* Now what has actually been contributed in these fields since our landmark, *The Abilities of Man* in 1927? As among the very first achievements of the kind we may take the research of Stephenson(3). For this work effected—besides other things—an extraordinary reduction in the sizes of the errors involved. First and foremost came those of sampling, which of course depend chiefly on the number of experimental subjects. Of these he managed to include no less than 1037, whereas the majority of previous experimenters had only had about 100 each. Another trouble affecting most of the older work, but happily escaped by Stephenson, had been that of excessive heterogeneity, especially in respect of sex and age. To obviate this influence, all his subjects were girls mostly between 10 and 12½ years.

Yet another danger had arisen from the arbitrariness and diversity of the scales in which the various test-scores had been respectively expressed. This disturbance he met by converting all the scales employed into such as to give standard normal distributions. And similarly all other known sources of error(4) were reduced to their minimum magnitude. Where feasible, the disturbers were eliminated. And when this could not be done, they were at least taken duly into account.

By all this corrective procedure, an extremely high standard was set up which had to be attained before the hierarchic equations could be admitted as fulfilled. Yet even so, they were successful thirteen times out of fourteen. And the author goes on to say that: "Neglecting this single (and readily explained) specificity gives an observed probable error of 0.0081 for a theoretical error of 0.0095." Indeed the author goes on to conclude that his results show "good agreement" with the "Theory of Two Additive Factors".

(2) *Contributions of Brown and Stephenson.* Great, however, as was in many respects the superiority of the foregoing research over all previous and even most subsequent ones, there still remained other respects not yet immune from criticism. In particular, one reproach made was the absence of evidence as to how the distribution of the tetrad differences is related to the whole error curve.

Another objection was that the main part of the experiments had dealt with too few tests, only six; whereas there were now said to be needed some twelve to fifteen. On both counts the most emphatic critic was Pearson. Much more guarded was William Brown. By the happiest arrangement, however, Stephenson and Brown proceeded to join hands (5). In a further investigation the latter made himself responsible for the mathematical arguments and conclusions, including the curve-fitting. His partner devised and applied the tests, besides calculating the correlations and the tetrad differences.

This time, whilst the subjects only sank to the still very considerable amount of 300 boys, the number of tests sprang from six to no less than twenty. These were spread over as wide a field as possible. The set was composed as follows: 9 were "primarily perceptual",

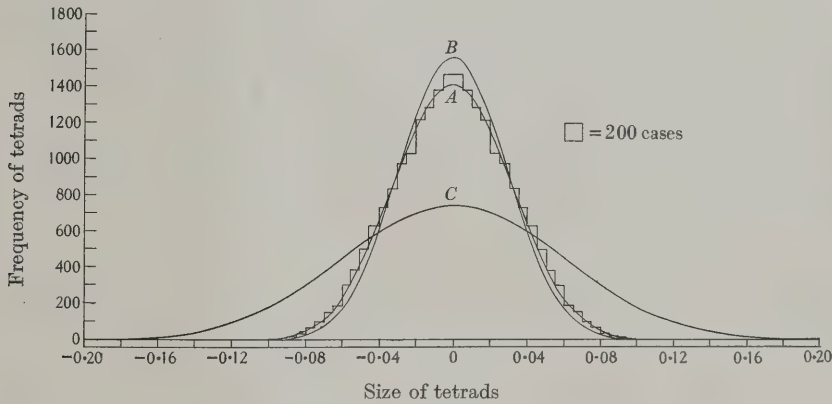


Fig. 1. (A) Curve of the tetrads actually observed. (B) Curve of the tetrads to be expected from the Theory of Two Factors. (C) Curve to be expected from theory of 'sampling'. Clearly, (A) agrees very closely with (B) and not at all with (C).

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5 more, though "not perceptual" were still "not verbal"; another 6 did have "a verbal constituent", but this was eliminated by the method of partial correlation. In this way, the number of correlations rose from 15 to 170. As for the tetrad differences, these came to no less than 14,535. As for the results, the whole of this immense material—with two very small and explicable exceptions—was found to satisfy the hierarchic conditions admirably. The chief values at issue were those of the standard deviations. Theoretically this amounted to 0.028, whereas the actual observed value was 0.031. Accordingly, Brown himself writes: "We may therefore conclude that, so far as this one frequency-constant (i.e. 0.031) is concerned, the criterion in the Theory of Two Factors satisfactorily passes the test of experience." And to this irreproachable agreement of the main algebraic statistics was added an equally close fitting of the curves, as may be seen in the curves in Fig. 1 (see (6)).

In conclusion Brown himself ended by writing: "The main purpose of our research has been achieved, namely to establish Prof. Spearman's Theory of Two Factors on an adequate statistical basis."

(3) *Contribution of Alexander.* A couple of years later appeared a third outstanding work, that of Alexander(7). But whereas the other two had been mainly conspicuous for their exactitude, this one excelled principally in respect of comprehensiveness. Thus the subjects presented the utmost diversity of age, sex and social standing, these characters being furthermore no longer thrown together, but instead distinguished and contrasted. As regards method too the hierarchic and the centroid were both utilized. Again, instead of the previous dominance of the scholastic verbal factor, we now find a more adequate appreciation of the factor he calls 'practical'. Moreover, all this experimental investigation was rendered pregnant by virtue of being framed in its educational, industrial and social context.

What then has been the issue of this unusually extensive and judicious research for our present problem, that of finding 'g'? The author concludes definitely enough: "In the Spearman tests of 'g' we have measures of the common factor, not merely of a battery of tests, but of the whole cognitive sphere. That being so, we are more than happy to accept the existence of 'g' as meaning the existence of 'the common intellectual factor'."

(4) *Contributions of Holzinger and his partners.* Let us turn to a fourth cardinal contribution to our present problem. It exemplifies the recent tendency to extremely wide range. As regards its authorship, this was very composite: the financial and moral inspiration came from Thorndike; the tests, nearly a hundred, were planned by the present writer; they were administered by several selected experts to about eleven hundred children; and all the results were elaborated statistically in nine successive booklets by Holzinger, of which the last appeared in 1936.

In this mammoth research, there was even less endeavour than before to obtain any ubiquitous fulfilment of the hierarchic equations. For, on the contrary, just those tests had been picked out or constructed which were most interesting on account of their fulfilment being most open to doubt. Moreover, use was here made of the new 'bi-factor' method, which expressly implies the fulfilment of only a certain number of the said equations, not of the remainder(8). Accordingly, in the former cases (though not in the others), the observed median and the calculated probable error turned out to have exactly the same value.

Not content even with this vast work, however, Holzinger—this time partnered with Swineford—published in 1939 another one which carried our problem a step still further(9). This time the main character to be called into question about all these factors was the vital one of stability. This point had already been raised in the big work just mentioned, and it had been already answered in the affirmative. An analysis made of sixty-three tests had revealed nine common factors. Then thirty-six of the tests had been selected for re-analysis by the same method and the two sets of results had proved to be—with certain exceptions—in very close agreement. Turning now to the newer work of Holzinger and Swineford, this time the stability was tried still more highly. On this second occasion several new tests had been introduced. Moreover the subjects on the two occasions were different (though of the same general type). The issue was again on the whole positive. The authors came to the conclusion that "remarkable stability was found for the same test battery and the same objective method of analysis".

From these same two authors, however, came yet another fundamental research(10). This time, the matter of inquiry was not so much theoretical as practical. They wished to

examine their factorizing method as a 'working tool'. For this purpose they wanted to bring the test-scores into relation with various school studies, life occupations, and character traits. Above all, they desired to ascertain how far the reliability and the validity of the results is affected by reducing the number of tests from twenty-eight to thirteen and therefore correspondingly economizing the time of their administration. The subjects numbered 457, about half being of each sex. On all the major points, the results of Holzinger and Swineford—guided as they were by a long succession of previous preparatory studies—showed the factor '*g*' to possess the desired stability and consistency in full measure. The general conclusion, once more, was affirmative. As in all the other researches quoted above, so here too was found a consistent and reliable '*g*'.

III. VARIOUS

(1) *Further acceptances of 'g'*. Among yet further endorsements of '*g*' an interesting group is presented by those who, like Kelley⁽¹¹⁾ and even Thomson⁽¹²⁾, have been depicted as arch opponents of '*g*', whereas really they had accepted it definitely enough and had only preferred their own special interpretations of it. "On the whole the two sets of findings (Kelley's and Spearman's) are quite remarkably in harmony, the agreements being in the matter of a spatial, a numerical, a memory, and even a general factor, though this last is differently interpreted⁽¹¹⁾." "With this view (of Spearman), the present author has always agreed, provided that '*g*' is interpreted as a mathematical entity only, and judgment is suspended as to whether it is anything more than that⁽¹²⁾."

Another group of authors consists of those who, like Brown, and subsequently Piéron, had originally been chary of admitting '*g*' but eventually had by their own further studies been won over to accept it⁽¹³⁾. Even so, however, our list of the recent supporters of '*g*' as being at least a statistical entity is far from exhaustive. Among the more important cases may be quoted the following: Arsenian⁽¹⁴⁾, Clarke⁽¹⁵⁾, Wynn Jones⁽¹⁶⁾, Jorgensen⁽¹⁷⁾, El Koussy⁽¹⁸⁾, Line⁽¹⁹⁾, McManama⁽²⁰⁾, Monaghan⁽²¹⁾, Price⁽²²⁾, Reinhart⁽²³⁾, Ebbelwhite Smith⁽²⁴⁾, Milton Smith⁽²⁵⁾, Wright⁽²⁶⁾.

(2) *General factors other than 'g'*. Up to now, however, our account of the factors found in ability has always included a general one. But a few authors have explicitly claimed that *their* general factors are not '*g*' at all. Turning to consider these exceptions, we must first of all discount those cases where the designation of 'general factor' appears to be derived, not from the result of the analysis, but instead from its method (a practice that appears to the present writer confusing). "I shall call those methods which proceed by analysing the correlation matrix taken always as a whole 'general-factor methods', and those which partition the correlation matrix into suitable sub-matrices, and then analyse these separately, 'group-factor methods'"⁽²⁷⁾.

Passing on to the cases where the alleged general factors are so in the ordinary sense of the word, these appear to be almost confined to the two long familiar ones; either Hotelling-Kelley's 'Principal Component'; or else Thurstone's 'Centroid'. And even these two, though truly enough not identical with '*g*' are usually at any rate very good approximations to it.

Accordingly, the great majority of authors using either of them—especially the centroid—usually have done only so in combination with, and so checked by a determination of '*g*' also. Notable instances have been Alexander⁽⁷⁾, Clarke⁽¹⁵⁾, Flanagan⁽²⁸⁾, Price⁽²²⁾, Ebbelwhite Smith⁽²⁴⁾ and Milton Smith⁽²⁵⁾.

Very different is the case of what Burt⁽²⁷⁾ has called the 'bipolar factors'. These do present values widely unlike those of '*g*'. Essentially they are characterized by not being limited to positive values, but instead ranging freely throughout all values positive and negative. And if up to now they have attracted comparatively scant attention, this is because they have in truth offered little scope for use in the chief field of labour of factorization, namely abilities, which are essentially positive. But they have quite recently been coming more and more into their proper estate, as research is increasingly turning to that other great side of human nature which is orexis (feeling and volition).

(3) *Absence of general factors.* There will remain to consider, especially by way of contrast, the cases of absence of any general factor at all. The most striking feature about these is their extreme rarity. The present writer is acquainted with only two of them. And even to these some objections have been made. The case brought forward by Guilford contains, if not a general factor, at least a very good imitation of it. The case of Thurstone, on the other hand, is definite enough, but it suffers from the reproach of having been elicited, not by any character of the actual data at issue, but only as an arbitrary *tour de force*. And even so, its very possibility was shown to have been largely due to the excessive size of his experimental errors⁽²⁹⁾.

Nevertheless cases, even if not yet realized can at least be conceived, where a factorization without any general factor is irreproachably demonstrated, and might even be scientifically serviceable. Even here, however, the evidence has gone to show that such a factor was not so much really absent as only latent. By means of appropriate statistical procedure it could be immediately revived in full force.

IV. UPSHOT

We have marshalled the recent evidence as to whether or not the general factor designated as '*g*' may be properly said to exist. By this is meant that it presents at least the cardinal statistical characters—definiteness, comprehensiveness and stability—which are indispensable for the purposes of science.

The answer, recorded in an immense amount of such research as that of Stephenson, Brown, Alexander and Holzinger, has been a decided affirmative. Accordingly we appear now entitled to proceed to what has been found about its properties.

B. INTERPRETATION OF '*g*'

I. FUNDAMENTAL METHODS

(1) *Blind coefficients.* First and foremost among such properties we may notice that so far in our account the information about '*g*' has been limited wholly to its quantity irrespective of its quality. This holds good in particular of the vital 'saturation' or 'loading' coefficient r_{xg} . For this only indicates how much of the '*g*' at issue exists in '*x*'; it has been blind as to *what* exists thus much.

(2) *Original suggestions.* Now in default of any better explanation, the present writer originally contented himself with some suggestions more illustrative than informative. Thus on the psychological side, it was said, the chief influence governing a person's '*g*'-score might perhaps be his intelligence; or else his concentration, or his will power, or his mental energy. On the physiological side his score might perhaps be determined by his

neural plasticity, or the state of his blood, or his neural energy. Another suggestion hazarded that of mere chance. Again, the influence really operative might be highly complex, or even multiple. Turning to another aspect of the matter, the inward nature of the factor, the sought influence might be essentially causal, or else merely sequential.

(3) *Method of agreement and difference.* Few, however, if any, of all such suggestions would appear to have secured much support in the way of definite evidence. The simplest—and crudest—step in this direction is afforded when any factor to be explained and any explanatory test character are found to be highly inter-correlated. As an (imaginary) illustration the factor ‘*g*’ might in some set of tests turn out to be highly correlated with the test character of spatial discrimination. From this observation the inference might be drawn that the two, ‘*g*’-factor and spatial discrimination, are mutually explanatory. But any such inference would, to say the least of it, be precarious. For instead, the observed correlation *might* derive, not from the spatial character of the test, but rather from some other feature of it; perhaps, that of being perceptual.

To guard against this contingency, more cogent evidence could be obtained by ascertaining that such a high correlation between the ‘*g*’-factor and the spatial character is to be found repeatedly. But even this enrichment of the evidence is not enough for security. For possibly the spatial character has in such sets of tests so very wide a range that its correlation with other characters may be extremely frequent, and therefore not really distinctive at all.

To clinch the evidence, proof may be finally supplied not only that the presence of the spatial factor and that of a high ‘*g*’ tend to go together, but also, inversely, that absence of the spatial character entails likewise the absence of high ‘*g*’. But in this fashion we appear to have been at last brought back to the classical method of research designated by J. S. Mill as that of Agreement and Difference.

II. RELEVANT EXPERIMENTS

(1) *Older work.* From the description of the method by which we propose to treat our problem, let us now turn to the chief data available to be so treated.

Among the older studies that might at first sight be considered for the purpose, the earliest in the field would appear to have been the outstanding research of H. Peterson (30), who in 1908 submitted sixty-three university students to five tests entitled respectively: Reasoning, Generalization, Abstract Thought, Memory and Accuracy. For its results, see Table 1. Among other distinguished works in the same sphere may be specially mentioned those of Burt (31), Stockton (32) and Herring (33).

Table 1. *Correlations of Peterson*

Nature of tests	1	2	3	4	5
1. Reasoning	—	0.95	0.83	0.40	0.45
2. Generalization	0.95	—	0.86	0.40	0.28
3. Abstract thought	0.83	0.86	—	0.64	0.48
4. Memory	0.40	0.40	0.64	—	0.31
5. Accuracy	0.45	0.28	0.48	0.31	—

Still on closer inspection none of these studies, however valuable for their own needs, appears to have supplied just the factorial data which we are now requiring, namely data which would enable us to apply the method of Agreement and Difference.

(2) *Unitary traits research.* Passing on to recent times, we come upon the extremely

ambitious work called from its topic that of 'Unitary Traits' (34). This study has already been mentioned by us above, as contributing to prove that 'g' exists. It will be here cited again, this time for the information it supplies as to how this same 'g' may best be explained. For this latter purpose it is especially suitable on account of its extremely wide and systematic range of cognition apprehended. For these characters render it exceptionally amenable to our chosen method of Agreement and Difference. To begin with, the tests were devised in two main classes. Of these the first was primarily of theoretical significance, and so aimed at the greatest simplicity and fundamentality. The other class aimed more at contact with ordinary life, and so was for the most part inclined to be more complex; the tests were intended to serve the double purpose of verifying the theoretical conclusions and of utilizing them. (For list of traits, see Appendix.)

The former or theoretical tests, (a), were in part directed towards salvaging whatever might be of value in the time-honoured 'faculties'. Groups of tests were devoted respectively to studying Perception, Intellect, Memory, Imagination, Attention and Voluntary Movement. Each of these, again, furnished material for sub-groups. For instance, the Perception group was given one for sight, one for sound, and so on. Again, the Intellect group included Reasoning, Abstraction and Generalization. Memory was partly 'logical' and partly 'mechanical'; again, partly 'immediate' and partly 'delayed'. In the case of Imagination, the sub-groups included 'controlled' and 'free'; also 'visual', 'auditory' and 'verbal'.

But in addition to all such guidance of the tests along the lines of the ancient doctrine of faculties, others still more important took into account the chief modern theories of cognition, notably the one which is based on the idea of noegenesis.¹ Thus the perceiving of relations was contrasting with the inventing of 'correlates'. Further, the different kinds of relations were compared with one another. Yet more tests were so constructed as to illuminate the various quantitative characters: clearness, complexity and speed; span, retentivity, fatigue, conation and 'primordial potencies'; also perseveration and oscillation. Finally, numerous tests were specially devised to elucidate not so much the essential purport of the tasks involved as, rather, certain more or less accidental influences of the manner of presentation and execution.

Then came the second general class of tests, (b), that by which it was hoped to keep nearer to ordinary life. These supplied three large groups which may be roughly designated as Scientific, Artistic and Psychological. The first of these, again, presented three main sub-groups for Arithmetic, Geometry and Mechanics; and these, especially Mechanics, were then further divided. Turning to the artistic tests, these were in the first place subdivided into literary, graphic and musical. The further grouping of the literary tests distinguished Comprehension from Expression. The former, again, dealt separately with letters, grammar, composition and still higher integrations. An approach to the psychological ability was sought by tests dealing respectively with emotion, behaviour and society. Interesting additions were tests of information, as also some of the tests commonly employed to determine 'Intelligence Quotients'.

(3) *Thornton School*. Out of the preceding work on 'Unitary Traits' there were successively developed by Holzinger, Harman and Swineford three other studies, closely akin.

¹ This word has been coined out of the Greek *nous* and *genesis* to designate the creation of knowledge in its two chief forms, the eduction of relations and that of correlates. For further details see the writer's *The Abilities of Man* (2), chap. XII.

These, though far from emulating its grandiose scope, possessed instead continually increasing technical exactitude and rigour. For our present purpose, however, we need only consider the last of the three; the one which was executed at Thornton School (34). For this was in full agreement with the other two, besides having many more subjects (457), as compared with the Pasteur School (156) and the Grant-White one (145). As regards number of tests, all three later schools had about two dozen, and so much fewer than the Traits research.

III. PRIMARY EVIDENCE

(1) *'Intelligence'*. After all this clearing of the ground, the actual solution of our problem becomes simple enough. It consists, as we have seen, in satisfying a two-fold condition. The first clause is that there has to be discovered some character such that its dominant presence in any of the tests at issue entails always a high loading of that test with '*g*'. The second clause is reversed; now the *absence* of the said dominant character must always entail only a *small* '*g*'-loading. Our plan of operations will usually consist in a try-out; we will take a few of the characters that seem to be at any rate tolerably plausible.

One such suggestion might conceivably be in the character of '*intelligence*'. But this at once breaks down because of its equivocality. How can we even begin to see whether '*intelligence*' is present or absent in the tests so long as nobody knows what this word is really intended to mean?

(2) *Intellect*. Far superior in this respect of definiteness, at any rate, is the otherwise kindred concept of '*intellect*'. For about the significance of this term there is comparatively little dispute, so that we can at least give it a trial.

Now commencing with the positive clause of the two-fold condition, we do find this satisfied well enough; all tests dominated by the concept of Intellect in any of its acknowledged forms do actually show high '*g*'-loadings. In the case of the results of Peterson, this agreement is very plain. Passing on to the results of the '*Unitary Traits*' study, the principal tests at issue are (11) Evidence relations, and (82) Generalization (see Appendix), and the loadings of these respectively show the high ranks of 7th and 2nd out of the total of 41. Similarly striking are the results yielded by the Thornton School. For here the outstanding intellectual tests are (23) Deduction of letters, (25) Problem reasoning, and (28) Deduction of figures. And their respective ranks, out of 28 are as high as 4th, 3rd and 5th.

Quite otherwise, however, are the results when we pass from the positive to the negative clause of the condition; from that of agreement to that of difference; from the '*intellectual*' cases where the '*g*'-loading ought to be high, to the non-intellectual cases where it ought on the contrary to be low. To the latter category belong first and foremost all the tests of sensory perception. And yet far from these all being low, one of them in the '*Unitary Traits*' results takes the highest rank of all the forty-one. A still more fundamental consideration in failure to satisfy the negative clause is that in all cognition legitimately termed '*intellectual*' the crucial relation is always that of evidence; that is to say, something is taken to '*prove*' something. But the high loadings, far from being confined to this evidential kind, appear to be just as high with relations of any of the other nine kinds. In the '*Unitary Traits*' results compare for instance the tests (11) and (13). The former explicitly tests the relation of evidence; the latter that of likeness. Yet the

latter has, if anything, the higher loading of the two. If we turn to the Thornton School, the picture is, if possible, even more conclusive. High as we have seen to be the loading of the evidential tests (23), (25) and (28), at least as high are many of the arithmetic, which depend on the relation of 'conjunction', (24), (26) and (27).

(3) *Noegenesis*. Yet another plausible suggestion would be that of noegenesis. For this does satisfy the negative clause; absence of dominant noegenesis obviously does entail absence of large loadings. But on the other hand, it breaks down at the positive clause; presence of dominant noegenesis does *not* necessarily involve presence of large loadings. An example of such failure of the positive clause is afforded by most tests of sensory perception. Thus in the 'Unitary Traits' study the tests (19) Pitch discrimination, and (6) Brightness discrimination, are definitely noegenetic and yet their loadings are so low, as to have only the ranks of 27th and 21st out of 28. With the Thornton School, similarly, the noegenetic perceptual tests (2) Cubes, and (3) Paper form board, have only the low ranks of 18th and 20th out of the 28.

(4) *Abstractness*. A more promising candidate to fit the bill might seem to be the character of abstractness. But on actual trial this too shows itself to satisfy only the negative clause, not the positive one; in general the latter breaks down whenever the abstract idea is merely reproduced, not educed. The 'Unitary Traits' study supplies an extreme example in test (87); this deals solely with abstract ideas, but is only reproductive, and so has a load little over zero.

(5) *Combination of conditions*. But the preceding suggestions, although singly so inadequate for the satisfaction of our two-fold condition, may yet conceivably be able to cope with it in combination. Let us give them a trial, taking in particular the combination of noegenesis *with* abstractness.

Commencing with the positive clause, we find that the 'Unitary Traits' study shows the combination to be present in six of the tests, (11), (13), (17), (18), (78) and (82). As for their ranks, these (arranged in order) are 2, 3, 6, 7, 8 and 10. That is to say, *all* lie on the uppermost quartile; the agreement is flawless.

Turning to the Thornton School, here the results are still more striking. In this school, again, six tests present the said union of noegenesis with abstractness; they are (26), (24), (25), (23), (28) and (27). On the other hand, their respective ranks prove to be 1, 2, 3, 4, 5 and 7; that is to say, once more all are within the uppermost quartile.

We turn to the negative clause of the condition of correspondence. Do all the tests which *fail* to present the union of noegenesis with abstractness fail also to have high loads? This time the agreement is not quite so perfect. For although the 'Unitary Trait' study presents nine tests which fail to present the said union, only seven of these fail also in respect of the loads, these having the ranks of 29, 27, 21, 40, 39, 41 and 36. The remaining two tests, (2) and (3-4), have on the contrary the very high ranks of 1 and 4.

Moreover the Thornton School produces just the same results. Out of six tests which fail to present the required combination, only five fail also to have high loads; their ranks are 18, 20, 12, 16 and 14. The remaining case, (1), ranks as high as 6. Moreover, this exceptional test is of just the same kind as before; namely the perception of spatial relations.

On the whole, then, this combination of noegenesis together with abstractness does satisfy the '*g*'-loadings *almost* perfectly. There remains but a single exception; this however is manifested in both our two sets of results, those of the 'Unitary Traits' study

and of the Thornton School; it derives each time from this remarkable test of spatial perception.

IV. SUBSIDIARY EVIDENCE

(1) *Corroboration and supplementation.* In the preceding section we have selected for trial what seemed to be the chief claimants to constitute the required psychological characterization of 'g'. Our method aims to be the best possible; that of 'agreement and difference'. And the quest may be said to have been successful. With one single exception, the whole of the available data have harmoniously indicated that 'g' combines abstraction with noegenesis.

The present section is to be more of a corroborative and supplementary nature. It aims at gathering and focusing any illuminating side light, so that nothing helpful should be overlooked. In particular, the fullest possible recognition should be accorded to any serious difficulties.

(2) *Anomaly of spatial perception.* Outstanding among all such troubles is the one we have just encountered about spatial perception. It is the fact of finding this ability, in spite of *not* being abstract, nevertheless giving at least one very high 'g'-loading, thereby seeming to conflict with all other results.

Nor can this paradox be attributed to any kind of accident or chance. For besides being much too large for such an explanation, it has a long history behind it. Originally, I believe, this test was invented by the present writer for the very purpose of measuring 'g'. Subsequently it was successfully employed for the same purpose by many authors, notably Zubin and Lorge. An avowed development of it has been perhaps the best of all non-verbal tests of 'g', namely, the 'Matrices' of Penrose & Raven (see 35).

Along quite different lines, but with fully concordant results, has been the work of a great many other psychologists, Stephenson, Slater and many others, on the spatial group factor.

But most explanatory and fundamental of all would appear to be the evidence of introspection. The present writer happened to notice that such tests can readily be performed in two distinct manners. One may be called analytic, in the sense that attention wanders from one element of the figures to another. The other mode of operation is comparatively synthetic, in that the figures (or their constituents) are mentally grasped in much larger units (sometimes called 'wholes'). The former procedure, not the latter, tends to load noegenetic processes with 'g'.

As for the testee's choice between the two modes of activity, this would appear to depend on various influences, such as habit, efficiency, sense involved, and so forth. The role played by the analytic procedure may be comparatively small, but it appears—with the human mind at any rate—to be never completely absent from anything likely to be employed as a test.

Incidentally, here may lie the real ground for the otherwise strange fact that 'g'-loadings are almost always found in some degree, not only throughout all tests of sensory, but even in those which are commonly taken to be of a psycho-motor kind.

(3) *Case of arithmetic.* Another case that may be singled out for special elucidation is that of arithmetic. For this ability there were five tests at the Thornton School. And of these, as we may see, three stand at the very top of all, the ranks being no less than 2nd (Numerical puzzles), 3rd (Problem reasoning) and 1st (Series completion). Indeed,

the authors themselves were so much impressed by the last of these, that they proposed in measuring '*g*' to drop everything else and to use this test by itself. Be this as it may, certainly these high loadings are in the best agreement with both of our requirements. For all three tests are plainly enough noegenetic. And what can be more 'abstract' than number?

But a great change comes over the scene when we turn from these three tests to another arithmetical one (12) entitled Addition. For now the rank drops from the very top to almost the bottom (22nd). However the upset is not surprising, seeing that this addition test, besides being largely one of speed, consists in nothing but continual repetitions and so is *not* noegenetic.

There remains one more arithmetical test done at this school; it is the 'Fundamentals' of Woody and McCall. Now this as regards its psychological character is definitely more repetitive and mechanical than the first three considered above, but less so than the fourth, which is Addition. Accordingly, its rank too is intermediate (7th).

Turning next to the 'Unitary Traits' study, here one of the arithmetical tests was again the Fundamentals. And its results were much the same as before.

Finally we reach two more arithmetical tests entitled Proportion and Permutation. But these were frankly experimental. They only aimed at presenting the opposite extreme to all the others in that they alone did not break up into aggregates of very familiar components, but instead constituted most unfamiliar problems. Though not really more difficult than the other tests, they made the impression of being more 'abstruse'. The interesting results of this change of character is that now the '*g*'-loadings were much *diminished*.

V. CONCLUSION

In general this paper has been devoted to bridging over the factorial gap between statistics and psychology. The principal data for this purpose have consisted of the 'saturation' or 'loading' coefficients. The principal method of procedure has been that of 'Agreement and Difference'. And the principal conclusion has been that '*g*' is essentially characterized by the combination of noegenesis with abstractness.

Among our more special topics, the most important have concerned the '*g*'-loading of spatial perception and of arithmetic.

APPENDIX

'Unitary traits' (34)

(a) Theoretical Units

Test no.	Loading	Rank	Description of tests
Perceptual relations			
1	0.542	29	Spatial relations, I
2	0.943	1	Spatial relations, II
3-4	0.780	4	Spatial relations, III-IV
5	—	—	Pitch discrimination
6	0.600	21	Brightness discrimination
Perceptual correlates			
7	—	—	Perceptual correlates indicated abstractly
8	0.701	12	Perceptual analogies
9	—	—	Brigham spatial, I
10	—	—	Brigham spatial, II
11	0.734	7	Evidence relations, I

Test no.	Loading	Rank	Description of tests
12	—	—	Evidence relations, II
13	0.769	6	Likeness relations, I
14	—	—	Likeness relations, II
15	—	—	Real relations, I
16	—	—	Real relations, II
Verbalized correlates			
17	0.813	3	Verbal analogies, I-II
18	0.710	10	Verbal completions, I-II
Reproduction			
19	0.512	27	Pitch, delayed discrimination
20	0.539	30	Unrelated association, code
21	—	—	Related association
22	0.566	26	Recognition, immediate
23	—	—	Recognition, delayed
24	—	—	Memory, tested by report
25	0.709	11	Memory, tested by questions
Psycho-motor ability			
32	0.239	40	Tapping
33	—	—	Dynamometer
34	0.268	39	Dotting speed
35	0.043	41	Maze speed
36	0.456	34	Handwriting speed
Mental speed (part of oscillation)			
42	0.571	25	Counting
43	0.465	35	Capitals
44	0.429	33	Code, parts
45	0.552	28	Adding (monotonous)
Perseveration			
48	—	—	I
49	—	—	II
50	—	—	III
51	—	—	IV
52	—	—	V
Attention			
53	0.515	32	I. Numbers and letters
54	0.620	18	II. Multiplication
55	0.641	17	III. Alphabet
56	0.683	13	IV. Addition
57	—	—	V. Attention
(b) Ordinary Units			
Science			
60	0.777	5	Arithmetic, fundamentals (Woody-McCall)
61	0.581	23	Arithmetic, problems (proportion)
62	0.679	14	Arithmetic, problems (permutation)
64	0.644	16	Geometry
67	0.575	31	Mechanical comprehension, I, pulleys
68	0.303	38	Mechanical comprehension, II, levers
69	—	—	Mechanical invention, I
70	—	—	Mechanical invention, II
Psychological skill			
71	—	—	Interpretation of emotional expression
72	—	—	Interpretation of behaviour
73	—	—	Solution of social problems
Linguistic comprehension			
74	0.394	36	Recognition of form
75	0.582	22	Recognition of letters
76	—	—	Recognition of words
77	0.580	24	Understanding of words
78	0.731	8	Understanding of paragraphs

Theory of general factor

Test no.	Loading	Rank	Description of tests
Linguistic expression			
79	—	—	Choice of words
80	0.720	9	Grammar
81	0.613	19	Composition
82	0.891	2	Generalization
83	—	—	Abstraction
Imagination, free			
84	—	—	Ink blots
85	—	—	Completing forms
86	—	—	Completing pictures
87	—	—	Verbal fluency
88	—	—	Realization of strange situations
Imagination, controlled			
89	—	—	Imitation
Aesthetic appreciation			
90	0.657	15	Musical appreciation
91	0.369	37	Musical intervals
92	—	—	Pictorial appreciation
93	—	—	Literary appreciation
General information			
94	0.611	20	Information

Thornton School

Spatial			
1	0.547	6	Visual perception test
2	0.408	18	Cubes
3	0.292	20	Paper form board
4	0.499	13	Lozenges
5	0.455	16	Flags
6	0.494	14	Designs
Verbal			
7	0.520	11	General information
8	0.540	9	Paragraph comprehension
9	0.474	15	Sentence completion
10	0.542	8	Word classification
11	0.506	12	Word meaning
Speed			
12	0.229	22	Addition
13	0.135	24	Counting groups of dots
14	0.367	19	Capitals
15	0.057	28	The 'X' test
16	0.120	27	The '3' and '7' test
Memory			
17	0.533	10	Figure recognition
18	0.263	21	Word recognition
19	0.127	26	Number recognition
20	0.142	25	Object-number
21	0.222	13	Number-figure
22	0.444	17	Figure-word
General			
23	0.557	4	Deduction (letters)
24	0.656	2	Numerical puzzles
25	0.616	3	Problem reasoning
26	0.872	1	Series completion
27	0.525	7	Woody-McCall (arithmetic)
28	0.555	5	Deduction (figures)

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SPEED AND ACCURACY OF WORK AS RELATED TO TEMPERAMENT

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I. *Introduction and survey of literature* (pp. 132–134). II. *Experimental procedure* (pp. 134–135). III. *Experimental population* (pp. 135–136). IV. *Results* (pp. 136–141). V. *Discussion* (pp. 141–142). VI. *Summary and conclusions* (pp. 142–143). *References* (pp. 143–144).

I. INTRODUCTION AND SURVEY OF LITERATURE

Speed and accuracy are two characteristics or qualities which enter into most of the activities of an individual. Speed can be defined as the rate at which an activity is performed, and accuracy as “that characteristic of measurable behaviour which is indicated by relative freedom from error—an error being considered as a deviation from an objectively correct standard” (Hartmann, 1928).

Popular belief considers speed and accuracy to be two antagonistic modes of reaction, either of which has a tendency to prevail in certain people when they are confronted with a variety of tasks. One commonly speaks of a quick, but slapdash, and of an accurate, but slow person.

The present investigation studies the interdependence of speed and accuracy in a number of tests, and attempts to examine the temperamental variables that are bound up with these two characteristics. For this purpose two groups of subjects, of whom the main temperamental traits are known, have been selected and have been given a number of tests involving a choice between performing the task with special attention to speed or to accuracy. These two groups consist of patients suffering from affective and hysterical disorders respectively. Descriptions of their personalities by psychiatrists suggest that they would react differently to these tests and that the hysteric would constitute the speed preference type, and the affective or dysthymic the accuracy preference type. A more detailed description of the experimental population will be given later.

Closely linked with the problem of the interrelationship of speed and accuracy is that of the generality or specificity of these two characteristics. If speed and accuracy be specific to a given task, it would not be possible to speak of a trait of speed or of accuracy and to investigate the temperamental qualities related to these traits.

Since no summaries on the problems of (a) the existence or non-existence of a speed factor, (b) the existence or non-existence of an accuracy factor, and (c) the interdependence of these two characteristics are available, the relevant literature will be briefly reviewed.

(1) *Survey of investigations on speed.* A large number of investigators have studied the problem of the presence or absence of a speed factor. The outcome is a rather confusing picture of contradictory results. Lanier (1934), Trow (1925), and Sisk (1926) found insignificant correlations between speed in simple and complex tasks. Studman (1935), Walters (1927), Kennedy (1930), Aitkens, Thorndike & Hubbel (1902), and Downey (1923), however, found positive correlations between a variety of tasks, both simple and complex. The investigators who obtained positive correlations between the rates at which different

tasks were performed can be divided into those who, with Bernstein (1924), believed that there is no speed factor independent of intelligence, but that speed is one aspect of intelligence; and those who, with Kennedy (1930), failed to find a significant relationship between speed and intelligence. Investigators who supported Bernstein's findings were Peak & Boring (1926), Rounds (1928), McFarland (1930), Clark (1926), Wyatt (1913). Those whose results agreed with Kennedy were Sisk (1926), Farnsworth, Seashore & Tinker (1927), Seashore (1930), Travis & Young (1930), Dowd (1926), McCall (1916), and Tinker (1944). Slater (1938) investigated the problem by noting the voluntary rate at which children answered the sub-tests of a battery of intelligence tests; he arrived at the conclusion that the preferred rate of work was independent of intelligence and consistent over a variety of tasks.

Some of the reasons for these very divergent results may be: insufficient number of subjects, different degrees of homogeneity of the groups tested, different measurements used.

A few facts, however, seem to have been established in this field. Different types of reaction times intercorrelated highly (Lanier, 1934), even when measurements were taken from various parts of the body. Distractions and complications in discriminative reaction-time experiments did not appreciably lower the correlations (Farmer & Chambers, 1926). Simple and discriminate reaction times correlated together moderately highly (Lemmon, 1927; Perrin, 1921; Kennedy, 1930; Lanier, 1934), but were not closely related to serial reaction time (Beck, 1932; Farnsworth, Seashore & Tinker, 1927; Hansen, 1922; Lanier, 1934). Tapping showed little relation to reaction times (Perrin, 1921; Ream, 1922; Reymert, 1923; Seashore, 1930). Serial motor responses showed low positive correlations (Lanier, 1934).

Intercorrelations of motor functions depended to some extent on similarity of postural preparation (Lanier, 1934).

Higher order mental functions gave high correlations (Gates, 1921; Jones, 1917; Sutherland, 1934); high intercorrelations of these functions have also been reported by Clark (1926), Courthial, van de Stadt & Claparède (1932), Garrison (1929*b*), Hunsicker (1925), Longstaff & Porter (1928), and McFarland (1930). Mental fluency tests intercorrelated highly (Studman, 1935; Notcutt, 1943); and were positively correlated with speed of association.

From this survey of the literature of the experimental data on the generality or specificity of rate of performance, it will be seen that it is difficult to find a single concept to explain these contradictory results. It appears, however, that sufficient evidence has been provided to postulate the existence of one or more speed factors.

The author is of the opinion that several speed factors exist which are relatively unrelated to one another. The assumption of a number of speed factors would account for the high intercorrelation of the reaction-time tests on the one hand, and of the mental fluency tests on the other.¹ Tasks of the sensory-motor type appear to lie half-way between the semi-automatic and the eductive tests and to intercorrelate with one another to a moderate degree.

(2) *Survey of literature on accuracy.* The experiments that have been carried out with reference to the specificity and generality of accuracy are far fewer in number. The most

¹ A. Petrie (1945) showed that eight tests of fluency intercorrelated significantly when intelligence had been partialled out.

important of these was the investigation of Hartmann (1928). He used eleven different tests and found that accuracy did not constitute a functional unit of behaviour. "In general the results indicate that there is no ground for the belief that an individual's accuracy of performance in a certain group of tasks enables us to predict his accuracy in other fields of work." This conclusion is qualified by the type of tests used and by the methods employed. In view of the contradictory results that have been obtained with respect to personal *tempo*, it appears that more investigations are needed to confirm Hartmann's findings. Others have studied the interdependence of speed and accuracy, but unfortunately made no attempt to establish the unitary nature of either tendency (Mudge, 1921; Thorndike, 1915; Longstaff & Porter, 1928; Burt & Fry, 1934; and Tiffin, 1943).

(3) *Survey of literature on the interdependence of speed and accuracy.* Thorndike (1915) and Mudge (1921) found that the quicker subjects were also the more accurate. Longstaff & Porter (1928) obtained insignificant correlations between speed and accuracy, the average intercorrelation being 0.07. Sturt (1921), in an investigation of methods of teaching typing, found an inverse relationship between speed and accuracy. These findings were confirmed by Burt & Fry (1934). Using a number of rather ingenious tests they produced evidence for a unitary trait, 'recklessness', which appears to be nothing more than a preference for speed over accuracy. Tiffin (1943) obtained a correlation of -0.49 between speed and accuracy for tin-plate inspectors.

Reviewing these investigations on the interdependence of speed and accuracy we find again a number of contradictory results, but several broad trends emerge. Tests in which intelligence plays a predominant role (Thorndike, 1915) showed a positive correlation between speed and accuracy. Tests where no intelligence is involved, and where the tasks are primarily of a manipulative character, intercorrelated negatively for speed and accuracy (Burt & Fry, 1934; Tiffin, 1943). Low and insignificant correlations were obtained when tasks were not of the manipulative type involving a moderate degree of intelligence (Longstaff & Porter, 1928).

The present investigations were undertaken in order to contribute to the clarification of the three problems reviewed above. In addition it was considered that if the investigations would show speed and accuracy to be generalized tendencies, the personality characteristics related to these tendencies could be studied profitably by a comparison of two neurotic groups of whom the main temperamental qualities were known.

II. EXPERIMENTAL PROCEDURE

After some preliminary experimentation, five tests were selected. The criteria for selection were: (a) diversity of tasks, (b) tasks which allowed for a sufficient spread of scores both with regard to the number of errors made and the amount of time taken, and (c) ease of administration. The latter consideration excluded the use of elaborate machinery and reduced most of the tests to the paper and pencil variety.

(1) *Hidden word test.* This was based on the one used by Hartmann (1928). The subject was directed to underline all common English words found in paragraphs of printed material of three letters. Hartmann found that "accuracy in underlining hidden words is practically unrelated to gross efficiency in so doing".

(2) *Cancellation test.* A sheet containing isolated letters in which A and U were to be crossed out alternately. It was found that the crossing out of one or two letters individually without specifying the order did not allow for a sufficient variation in the number of errors made.

(3) *Addition test.* The subject was presented with a series of two digit numbers and asked to add 7 to each.

(4) *Track tracer.* The test was originally designed by the Cambridge Psychological Laboratory, as a test of speed *versus* accuracy. An ivorine sheet covers a metal plate. On this sheet a path is marked out between rows of holes. The subject was instructed to trace the path with a metal stylus; each time the stylus touched a hole, it was counted as a mistake. The mistake was automatically recorded by means of an electric counter and in addition a buzzer was set in motion when the stylus made contact with the metal plate. The subject was thus warned each time he made an incorrect move.

(5) *Measurement test.* With a pair of dividers, set at a fixed distance, the subject was instructed to measure off equal divisions on a straight line.

The first four tests were each divided into two main parts. In the first part the test was explained to the subject. He was then asked to work *as quickly and as accurately* as possible in order to complete the first paragraph in the three paper-and-pencil tests, and to trace the path to the centre and back again to the starting-point in the Track tracer test. These tests will subsequently be referred to as 'choice tests', since it was left to the subject to weight the instructions in the direction of speed or accuracy in accordance with his special bias. Scoring was carried out: (a) with respect to the time taken, and (b) with respect to the number of errors made.

In the second part of the first four tests, and in the measurement test, emphasis in the instructions was placed on speed. A time limit was imposed and the subject was asked to show how much he could accomplish in a given time. The time limits for the different tests were chosen in such a way that the majority of subjects completed about half the task in the allotted time. Several of these tests, which will be referred to as 'speed tests', were given for each task. The following is a list of the number of speed tests and of the time limits imposed. (1) Hidden word test: four speed tests of 45 sec. duration each. (2) Cancellation test: four speed tests of 30 sec. duration each. (3) Track tracer: four speed tests, two of 60 sec., two of 30 sec. duration each. (4) Addition test: two speed tests of 30 sec. duration each. (5) Measurement test: two speed tests of 60 sec. duration each.

In the case of the measurement and the addition tests, only two speed tests were given because patients frequently resent tests with figures. The measurement test was given as a speed test only. On preliminary experimentation it had been found that the spread in the number of errors made was not sufficiently wide when no time limit was imposed. Errors in this test consisted in any inaccuracies in the size of the divisions as measured against the standard.

III. EXPERIMENTAL POPULATION

The tests were given to 100 neurotic patients of Mill Hill Emergency Hospital. These patients were twenty-five male and twenty-five female hysterics, and twenty-five male and twenty-five female dysthymics. The term dysthymic comprises patients suffering from anxiety states and/or depressive states—dysthymia being a convenient term to describe this empirically derived syndrome (Eysenck, 1944). The hysterical syndrome consisted of a constellation of hysterical symptoms with relative absence of overt anxiety or depression. Most psychiatrists stress the many elements that are common to both syndromes. They find, however, sufficient differentiating criteria to warrant the separation of the patients into these types. "Hysteria includes a number of reactions of a type which is distinctive both clinically and psychopathologically. Hysteria has a relationship with

anxiety states in that an anxiety state may in certain circumstances replace hysteria in a given patient, and a case may begin as an anxiety state and end as a hysteria. But these are not usual transitions. On the whole, in any one patient, the type of reaction tended to remain pure" (Henderson & Gillespie, 1940).

The patients were Service men and women, their ages ranging from eighteen to forty years. They were specially selected by the Medical Superintendent and his deputy as representing good clinical examples of these two main types of disorders. It is not claimed that the patients represented pure specimens of either syndrome. In view of the fluidity of some of the characteristics ascribed to either disorder, it is possible that in some of the cases psychiatrists would not agree amongst themselves upon the diagnosis. However, it is correct to say that, on the whole, in the hysterical group there will be a larger number of patients with a predominantly hysterical clinical picture, and in the dysthymic group a larger number of patients with characteristics predominantly of the anxious, obsessional and/or depressed kind. Moreover, any differences that we might find in the reactions of the two groups, would be *lowered* by an imperfect selection of the patients, but could not be artificially created by it. The difficulty of obtaining pure cases of either disorder thus attenuates the results to an unknown extent.

Previous work on these two reaction types had shown that they reacted differently on a number of objective tests such as intelligence tests, aesthetic preferences in the judgement of pictures (Eysenck, 1945), tests of persistence (Petrie, 1945), and tests of level of aspiration (Himmelweit, 1945). Psychiatric grouping of patients into two types which must be essentially of a qualitative character has therefore been confirmed by objective measurements.

The patients were given the Progressive matrices, a non-verbal test of intelligence (Raven, 1939). No difference between hysterics and affectives was found, their respective scores being 39.60 ± 3.00 and 37.52 ± 9.20 . The difference between men and women was, however, significant. The women were more intelligent than the men, their scores being 40.65 ± 8.25 and 36.36 ± 7.48 respectively. This difference was similar to that found in the general hospital population (Eysenck, 1945). It must not be attributed to any sex difference, but rather to the nature of recruitment into the Women's Services which is probably more rigid than that of the men.

IV. RESULTS

In the discussion of the results, data obtained on the choice tests and on the speed tests will be treated separately. The scores obtained on the separate parts of the speed tests were combined to obtain one speed score per task and subject. (The reliability coefficients of the speed tests were sufficiently high to warrant this procedure. Table 1 gives the reliability coefficients of the speed tests for the five tasks. The original coefficients have been corrected by means of the Spearman-Brown prophecy formula.) One score per task and subject was obtained for the choice test.

Since intelligence might be a factor influencing the rate and quality of performance, the speed and accuracy scores of the tests were correlated with scores obtained on the Progressive matrices. The correlations were run for the total group. From Table 2 it will be seen that, apart from the Word and the Addition tests, none of the other scores referring to the time taken, the amount completed or the number of errors made, correlated significantly with intelligence. In the case of the Word and the Addition tests the

correlations were significant, indicating that the more intelligent person took less time and made fewer mistakes in the choice tests and achieved more with fewer mistakes in the speed tests.

Table 1. *Reliability coefficients of speed tests*

	Speed	Mistakes
Word test	0.972 ± 0.001	0.931 ± 0.014
Cancellation test	0.862 ± 0.026	0.944 ± 0.012
Addition test	0.824 ± 0.033	0.823 ± 0.033
Measurement test	0.810 ± 0.035	0.821 ± 0.034
Track tracer (30 sec.)	0.788 ± 0.038	0.947 ± 0.010
Track tracer (60 sec.)	0.932 ± 0.014	0.949 ± 0.009

Table 2. *Correlations of speed and accuracy scores with intelligence*

	Choice tests	
	Time	Mistakes
Word test	-0.511 ± 0.074	-0.322 ± 0.090
Cancellation test	-0.190 ± 0.097	-0.213 ± 0.096
Addition test	-0.322 ± 0.090	-0.234 ± 0.095
Track tracer	-0.030 ± 0.100	0.191 ± 0.097
	Speed tests	
	Scores	Mistakes
Word test	0.610 ± 0.063	-0.131 ± 0.098
Cancellation test	-0.032 ± 0.092	-0.092 ± 0.099
Addition test	0.592 ± 0.065	-0.223 ± 0.095
Track tracer	-0.080 ± 0.099	-0.121 ± 0.099
Measurement test	0.001 ± 0.101	0.030 ± 0.100

Table 3. *Intercorrelations of speed and of accuracy scores for the four choice tests ($n=100$)*

Time	Word	Cancellation	Addition	Track tracer	First factor saturation
Word	—	0.64 (0.67)	0.43 (0.23)	0.54 (0.47)	0.84
Cancellation	—	—	0.23 (0.28)	0.15 (0.16)	0.63
Addition	—	—	—	0.30 (0.28)	0.41
Track tracer	—	—	—	—	$\frac{0.48}{0.38}$
Mistakes					
Word	—	0.22 (0.14)	0.46 (0.36)	0.15 (0.20)	0.34
Cancellation	—	—	0.50 (0.43)	0.32 (0.36)	0.48
Addition	—	—	—	0.64 (0.68)	0.90
Track tracer	—	—	—	—	$\frac{0.70}{0.41}$

In order to ascertain the degree of generality of the speed and accuracy factor as measured by these tests, correlations were run separately between the speed scores and the accuracy scores of the different tests. Table 3 gives the correlations for the choice test for speed and accuracy respectively, and Table 4 the equivalent figures for the speed tests. Intelligence was partialled out from these intercorrelations, the partial correlations being given in brackets.

A factorial analysis was carried out on the partial correlations, the results are given in Tables 3 and 4. It will be seen that both in the case of the choice and of the speed tests, a general speed and an accuracy factor were found. In the case of speed the factor accounted for 38 and 37% of the variance and, in the case of accuracy, the factor accounted for 41 and 24% of the variance. In these tests there was, therefore, abundant evidence to show that a person who worked rapidly on one test also tended to work rapidly on other tests and *vice versa*. Similarly, a person who was accurate on one test tended to be accurate on other tests.

Table 4. *Intercorrelations of speed and of accuracy scores for the five speed tests (n = 100)*

Score	Word	Cancellation	Addition	Track tracer	Measure- ment	First factor saturation
Word	—	0.29 (0.31)	0.41 (0.08)	0.59 (0.54)	0.62 (0.62)	0.65
Cancellation	—	—	0.35 (0.37)	0.31 (0.31)	0.33 (0.33)	0.53
Addition	—	—	—	0.34 (0.29)	0.35 (0.35)	0.42
Track tracer	—	—	—	—	0.42 (0.42)	0.65
Measurement	—	—	—	—	—	0.74 0.37
Mistakes						
Word	—	0.27 (0.26)	0.32 (0.29)	0.41 (0.39)	0.25 (0.25)	0.81
Cancellation	—	—	0.16 (0.14)	0.23 (0.22)	0.21 (0.21)	0.49
Addition	—	—	—	0.19 (0.16)	-0.18 (-0.17)	0.22
Track tracer	—	—	—	—	0.05 (0.05)	0.48
Measurement	—	—	—	—	—	0.17 0.24

Table 5. *Intercorrelations of time and mistakes for the choice tests and of score and mistakes for the speed tests. (Intelligence was partialled out and the partial correlations are given in brackets.)*

	Choice tests	Speed tests
Word	-0.04 (-0.24)	0.04 (0.15)
Cancellation	0.11 (0.16)	0.05 (0.05)
Addition	0.13 (0.07)	0.14 (0.34)
Track tracer	-0.38 (-0.37)	0.56 (0.57)
Measurement	—	0.10 (0.10)

The degree of interdependence of the two characteristics was examined. Table 5 gives the intercorrelations of time and mistakes for the choice and speed tests. As these intercorrelations may be affected by intelligence, the intelligence factor has been partialled out and the partial correlations are given in brackets. It will be seen that the two very significant correlations were those derived from the choice and the speed tests on the Track tracer. On this test, the quicker person was the more inaccurate, as shown by the negative correlation in the case of the choice test and the positive correlation in the case of the speed tests. The only other significant correlation was that of the speed part of the Word test. On the choice part of this test, the correlation failed to reach the required level of significance.

These findings are very interesting in view of the results obtained by other investigators.

The Track tracer being essentially of a manipulative character could best be compared to the tests of Burt & Fry (1934), and to those of Tiffin (1943). These investigators found a similar inverse relationship between speed and accuracy.

The results obtained so far provide sufficient evidence to warrant the assumption of the existence of a speed and of an accuracy factor. The temperamental qualities bound up with these factors will be subsequently examined by a comparison of the behaviour of hysterics and dysthymics on the tests.

On the whole the dysthymics were slower and more accurate than the hysterics. Table 6 gives the critical ratios of the means for speed and accuracy on the choice and on the speed tests. It will be seen that nine out of nine possible ratios for accuracy were in the

Table 6. *Critical ratios of the difference between the means of scores of dysthymics and hysterics for speed and accuracy*

(An asterisk has been placed next to those critical ratios where the general rule that dysthymics were slower and more accurate did not hold good.)

Choice tests		
	Time	Mistakes
Word	1.52	1.42
Cancellation	0.52*	0.14
Addition	0.33*	1.81
Track tracer	0.91	1.76
Speed tests		
Word	0.32	2.26
Cancellation	0.51*	1.51
Addition	0.13	1.85
Track tracer (60 sec.)	2.00	1.46
Track tracer (30 sec.)	2.51	1.87
Measurement	0.82	1.21

Table 7. *Critical ratios of the difference between the speed and accuracy standard scores that were obtained by dysthymics and hysterics respectively*

(In every instance the dysthymics were slower and more accurate than the hysterics.)

	Time	Mistakes
Choice tests	1.14	2.08
	Scores	Mistakes
Speed tests	0.63	2.53

expected direction. The difference between the two groups was, therefore, significant beyond doubt, even though not all the critical ratios obtained from individual tests reached the required level of significance. In the case of speed, the difference between hysterics and dysthymics was in the expected direction in six cases out of nine. The difference between the two groups was significant on one individual test only, namely the speed tests of the Track tracer.

In order to measure the extent of the difference between the two groups more clearly, their scores for speed and accuracy were turned into standard scores and added to one another. In this manner, one score for speed and one for accuracy were obtained for the choice tests and for the speed tests respectively. Table 7 gives the critical ratios for these standard scores. They were in the expected direction, the hysterics being quicker and more inaccurate than the dysthymics. The difference was not significant for the amount

of time taken in the choice tests and for the amount accomplished in the speed tests. The difference was significant, however, in the number of mistakes made in the choice and in the speed tests by the two groups.

From these results it can be seen that the behaviour of hysterics and dysthymics differs on tests involving special attention to speed or accuracy. The differentiation of the two groups with respect to accuracy was more significant than their differentiation with respect to speed. A measure of a person's accuracy is, therefore, a better measure for the investigation of the behaviour of these two reaction types.

A comparison of the scores of men and women, irrespective of diagnoses, was also carried out. The women tended to be quicker than the men. Their critical ratios are given in Table 8. The difference between the rate of performance of men and women was very significant, since most of the critical ratios were above 2.0. The only exception was found

Table 8. *Critical ratios of the difference between the means of scores of speed and accuracy that were obtained by men and women respectively*

(The general tendency is for the women to be quicker and more accurate than the men. An asterisk has been placed next to those critical ratios where this tendency does not apply.)

Choice tests		
	Time	Mistakes
Word	4.66	1.32
Cancellation	2.52	1.01*
Addition	0.20*	1.20
Track tracer	1.58	0.52
Standard scores	4.50	0.03
Speed tests		
	Score	Mistakes
Word	4.05	0.49
Cancellation	2.01	0.58
Addition	1.31	1.00*
Track tracer (60 sec.)	1.42	1.58*
Track tracer (30 sec.)	0.42	2.00*
Measurement	2.51	1.31*
Standard scores	2.52	0.82

on the addition test, where the difference was insignificant but the tendency reversed. The women tended to be more accurate than the men on a few of the tests, but the critical ratios were well below the level of significance. These results show that women tended to be quicker than men; any differences that were found between hysterics and dysthymics are not invalidated by these results, since each diagnostic group contained twenty-five subjects of either sex. It does, however, show the necessity for equating the sexes in an experiment of this kind.

In view of the inverse relationship found between the speed and accuracy of performance on the Track tracer, the scores of these tests will be transformed into indices that express the relationship of speed to accuracy. The following indices were obtained:

- (1)
$$\frac{\text{No. of mistakes} + 1}{\text{Time taken per sec.}} \times 100 \text{ for the choice tests}^1,$$
- (2)
$$\frac{\text{No. of mistakes} + 1}{36 - \text{actual score}} \times 100 \text{ for the speed tests.}$$

¹ 1 was added to the number of mistakes in order to obtain an index for those subjects who made no mistakes at all. The path of the Track tracer was divided into eighteen units and one point was scored for each unit traced. The total possible score was 36. A slow rate of work would thus lead to a low score and to a large difference between possible score (36) and actual score.

Table 9 gives the means and standard deviations for the choice test and for the combined speed tests. The difference between the means was significant in the choice test only (c.r. 2.04). On the speed tests, the index of the hysterics was larger than that of the dysthymics but not significantly so. The difference between men and women was not significant.

Table 9. *Index of accuracy to speed on the Track tracer*

		Hysterics	Dysthymics
Choice test	Mean	17.18	11.76
	S.D.	13.89	12.69
Combined speed tests	Mean	171.88	163.58
	S.D.	91.65	90.55

In view of the significant differences that were obtained for speed and for accuracy and for the ratio between the two on the Track tracer, this test can be considered to be the most discriminating of the tests used. It is interesting to do and quick to give, since the tracing of the path to the centre and back again to the starting-point (choice test) takes from 1 to 2½ min. The duration of the various speed tests is 3 min. The Track tracer is, therefore, the most promising test of this battery for the investigation of temperamental traits.

V. DISCUSSION

The findings of this investigation should be reviewed in relation to the results obtained by other investigators. By partialling out intelligence, it has been possible to study the phenomena of the generality and specificity of speed and accuracy in the absence of at least one of the many disturbing factors which have influenced work in this connexion. The existence of a speed and an accuracy factor has been demonstrated on tests which were given with equal emphasis both on speed and on accuracy, and also with special emphasis on speed in the instructions. The difference between the degree of generality of the speed and the accuracy factors which were obtained when subjects worked at maximum rates and at voluntary rates was not significant. These results were obtained from a neurotic population, and due allowance must be made for this fact in the interpretation of the findings.

For the speed factor, previous investigators can be cited in support (Studman, 1935; Walters, 1927; Kennedy, 1930; Aitkens, Thorndike and Hubbell, 1902; Downey, 1923; and Slater, 1938). Since it was possible to partial out intelligence before calculating the speed factor, these results confirm those of Kennedy (1930), Slater (1938) and others.

No literature can be quoted in support of the existence of an accuracy factor. Unfortunately the investigators who worked on the interrelationship of speed and accuracy (Thorndike, 1915, and others) had not studied the unitary character of either of these traits. The existence of a speed and of an accuracy factor can, however, be inferred from their results, since the same kind of correlations between speed and accuracy were obtained on a number of divergent tests.

The absence of significant correlations between speed and accuracy on the four paper-and-pencil tests is interesting. It is possible that an inverse relationship between speed and accuracy is only obtained on tests which are (a) essentially of the manipulative type,

and (b) of a type where the subject is immediately aware of any mistakes which he makes. On the Track tracer, for instance, where the quicker subject was the more inaccurate, a buzzer drew his attention to any inaccuracy and enabled him to adjust his speed to avoid future mistakes. Both Sturt's (1921) and Burt & Fry's (1934) tests were essentially manipulative and of a character which enabled the subject to appreciate immediately any inaccuracy in his performance. These investigators found an inverse relationship between speed and accuracy, similar to the one obtained on the Track tracer. On the paper-and-pencil tests, the mistakes which the subject made were not pointed out to him, and this in itself may be one of the factors responsible for the lack of a relationship between rate and quality of performance.

It would be important to confirm these findings of the interdependence of speed and accuracy on a number of manipulative tests.

In the second part of the investigation it has been possible to show that hysterics belong to the speed preference and dysthymics to the accuracy preference type. These findings are in line with those of Kennedy (1930), who produced statistical evidence for the separation of normal subjects on a speed test into two distinct types: "A rash, impulsive type that does everything quickly, and a slow, cautious type that does things slowly." Similarly, Bernstein (1924) stressed in his study of the rate of performance of school children, that some of the children were consistently fast and slapdash, whilst others were slow and careful. By using a neurotic population, it has been possible to bring out these differences more clearly and to extend these observations to accuracy as well as to speed. "It is sometimes easier to study a given trait in an exaggerated form, or to understand its role in the total personality when it is diminished below the usual strength...there is a tendency to-day to think of abnormal behaviour as normal behaviour which has become exaggerated, diminished or distorted" (Stagner 1937).

Hysterics have been shown to be quicker and less accurate than dysthymics. The tendency for hysterics to be quicker was less pronounced than that of being less accurate on a variety of tasks, in some of which mistakes were immediately evident (Track tracer), while in others no means of checking the accuracy of performance was available (Word test, Cancellation test, Addition test, and Measurement test).

These findings may be of interest, not only in the field of personality measurement, but also in the wider field of educational and industrial psychology, where knowledge of an individual's bias in his work towards speed or towards accuracy would be of some importance.

VI. SUMMARY AND CONCLUSIONS

1. Tests involving a choice between doing a task with special attention to speed or to accuracy were given to 100 neurotic patients at Mill Hill Emergency Hospital.

2. Four tests were of the paper-and-pencil type, the fifth was a manipulative test. The tests were each divided into two parts. In the first part the subject was instructed to work as quickly and as accurately as he could (choice tests), in the second part the emphasis in the instructions was placed on speed (speed tests).

3. A general speed factor was found after intelligence had been partialled out. The speed factor accounted for 38% (choice tests) and 37% (speed tests) of the variance.

4. In a similar manner, a general accuracy factor was found, accounting for 41% (choice tests) and 24% (speed tests) of the variance.

5. The interdependence of the two factors was examined by intercorrelating speed and mistakes for the various tests. Intelligence was partialled out. The resulting correlations showed no significant relationship between speed and accuracy on the four paper-and-pencil tests.

6. A significantly negative correlation between speed and accuracy was found on the Track tracer. As an explanation of the discrepancy in relationship between speed and accuracy on the paper-and-pencil tests and on the Track tracer, it is suggested that speed and accuracy are interdependent when the task is of a manipulative kind and one where mistakes are immediately evident to the subject. On the paper-and-pencil tests mistakes were not pointed out to the subjects.

7. The temperamental qualities related to speed and accuracy were examined by a study of the behaviour of fifty hysterics and fifty dysthymics. Each diagnostic group contained equal numbers of men and women, their ages ranging from eighteen to forty years.

8. Hysterics belong to the speed preference, dysthymics to the accuracy preference type. The difference between the scores of the two groups was suggestive in the case of speed and very significant in the case of accuracy.

9. The Track tracer was found to be the most discriminating test of the battery used.

10. Method of work, i.e. the speed and accuracy with which a task is performed, has been shown experimentally to be partly determined by a person's temperament. Tests of speed and accuracy of the type described in this investigation may, therefore, prove useful additions to a battery of temperament tests.

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LEARNING FROM GRAPHICAL MATERIAL¹

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I. *Introduction* (p. 145). II. *Subjects and methods of experiment* (pp. 145–147). III. *Results* (pp. 147–158): (1) *Difficulties in interpreting graphical material* (pp. 147–148); (2) *Difficulties in perceiving the charts and graphs clearly* (pp. 148–151); (3) *Difficulties in understanding terminology and concepts* (pp. 151–153); (4) *Difficulties in verbal formulation* (pp. 153–154); (5) *Difficulties in generalization* (pp. 154–156); (6) *The importance of a general background of knowledge and ideas* (pp. 156–158). IV. *Summary* (p. 158).

I. INTRODUCTION

The investigation described in this paper attempts to throw some light on the ability of adults, and especially adults who have not had a very wide general education, to understand and acquire information about problems of general social importance. It has recently been claimed rather widely that such adults, lacking as they frequently are in verbal facility, can grasp the implications of these problems more readily if the relevant information is presented to them graphically, in the form of charts, diagrams or pictures, and not mainly or entirely in verbal statements. The results obtained in this investigation seem to show that the acquisition of this information may be muddled and uncertain, if not definitely misleading, in so far as the learner possesses no definite or clear schemes of ideas or background of knowledge about the problems. When, as so frequently happens, his knowledge is limited to a random collection of vague ideas, slogans, clichés and prejudices, such factual information as is presented to him tends either (1) to remain unassimilated; (2) to be transformed into conformity with his pre-conceived ideas; or (3) to be reproduced in the form of isolated statements of fact not co-ordinated with the main tenor of his ideas, and having no influence in modifying the latter. Because he possesses comparatively little power of inductive reasoning, he is capable only to a slight extent of adding to his already existing body of knowledge about social questions, for instance, such new factual information as would extend, correct, elucidate and co-ordinate the former, unless careful education is given him in the acquirement both of general schemes of ideas and also of particular concrete factual knowledge.

II. SUBJECTS AND METHODS OF EXPERIMENT

There were three groups of subjects:

- (1) 52 Training college students (women); these had had a secondary school, but not a University, education, mainly in Arts subjects.
- (2) 89 R.A.F. aircrew in training; these men were in the main of good intelligence and technically skilled, but their general education was varied.
- (3) 90 men from a mixed army group, mainly 'other ranks' (N.C.O.'s and privates);

¹ Based on papers read to the Cambridge Psychological Society on 20 February 1945 and to the British Psychological Society on 8 April 1945. It is not possible in this paper to give full details of results or many illustrations of the material used; but it is hoped to include these in a book to be published shortly.

some of these were of good intelligence, but others, engaged on 'general duties', were of rather poor intelligence' and capacities generally.

Two classes of information were presented in the investigation:

(1) Data bearing on vital statistics, including population, birth and death rates and incidence of disease in various classes of the population. This information was largely unfamiliar to most of the subjects (students and airmen) who saw it.

(2) Data taken from the Government White Paper on 'Statistics Relating to the War Effort of the United Kingdom' (Cmd. 6564, 1944), showing the changes in occupation, production and consumption of the civilian population during the war. This information was familiar to the subjects (the army group and 23 of the airmen) in a general way, and also touched upon a good many preconceived ideas and prejudices.

Two types of graphical presentation were used for the data: (1) line and block graphs; (2) pictorial charts. In the later experiments data were also presented in tables of figures, by way of check.

There were two main methods of presentation, with certain minor variations: (1) the Training college students and 12 of the airmen were shown each graph or chart for 2 min.; at the end of this period the material was removed, and they had to report in their own words on what they had seen, the students in writing, the airmen orally. The answers were graded A, B, C or D, according to the degree of understanding indicated. Two difficulties were encountered in using this method: first, many of the subjects were lacking in verbal facility, and had difficulty in formulating their reports; and secondly, even when they had no lack of verbal facility, they often did not understand clearly what they were supposed to include in their reports. Thus in method (2), the remaining 77 airmen and the army group were asked series of questions after they had studied the graphs and charts, the questions being designed to test their recollections of the information contained in the graphs and charts. Some of these questions related to specific facts such as: the increase or decrease of population, or birth rate, or death rate, in certain classes of the population; or the increase or decrease of men and women in certain types of occupation since the outbreak of war. The answers to these were scored numerically according to their accuracy. Other questions were designed to test a more general grasp of the problem, such as: "If it is true that the children of certain countries will tend to become fewer in future, can you suggest how this decrease might be prevented?" (with instructions to "answer in the light of the information contained in the graphs or charts"). Or again: "What light does the information in these graphs or charts throw upon the alteration of civilian habits during the war?" The answers to these were graded A, B, C or D according to their correctness and comprehensiveness, and to the general understanding shown. Thus, A indicated full and comprehensive understanding; B, fairly good understanding, but statements not very coherent, or incomplete; C, one or two correct but uncoordinated statements; D, completely incorrect or irrelevant statements. In most of the results given below, grades B and C are combined, since they were often difficult to distinguish in practice.

Further variations of method were as follows:

(1) The students and airmen who had to report on the graphs and charts in their own words, had also at the end of the experiment to write a brief account of any general ideas or information they had gained from the material shown them.

(2) Eighteen of the airmen were also tested as to the result of answering the questions while actually

looking at the graph or chart. This modification did not seem to affect the results very appreciably, i.e. the difficulty of the experiment was caused by difficulties in understanding rather than in remembering.

(3) For 59 of the airmen and all of the army group, the information to be conveyed was arranged in three sets, and the subjects into three groups, thus:

Group I were shown set A in charts, set B in graphs, set C in tables of figures; Group II were shown set B in charts, set C in graphs, set A in tables of figures; Group III were shown set C in charts, set A in graphs, set B in tables of figures.

Thus the effect of difficulty in understanding and remembering could be isolated from the effect of variation in type of presentation.

(4) Each graph, chart and table of figures in the first set of data, on vital statistics, was presented for study for a period of 2 min. In presenting the data on the 'War Effort', the graphs, etc., were grouped in fours, each four relating to a definite topic: Mobilization of man-power, mobilization of industry and trade, alteration in civilian habits of consumption and spending. The subject was given 6 min., preceded by a short preliminary talk, to study each set of four, and was questioned at the end of the 6 min.

(5) Seventeen of the students were tested also with the N.I.I.P. Group test 33; and all the airmen and army group were tested with A.H. 4 (a written intelligence test containing both verbal and non-verbal material).

III. RESULTS

(1) *Difficulties in interpreting graphical material*

It is not often sufficiently recognized by those who advocate visual methods of presentation that the graph and the chart are no more immediately representational and no less symbolic of the information they are intended to convey than are verbal and mathematical statements. But whereas nearly everyone in the course of their upbringing acquires some facility in making verbal statements of ideas and meanings, only the specially educated learn to interpret general factual information from graphs and charts.

Consequently it is not surprising that this interpretation was often found to be difficult and the subjects were slow to make it spontaneously without explicit instructions. In particular, there was a tendency to describe the 'appearance' of the graphs or charts rather than to report on the information they were intended to convey. Thus 31% of the students' reports and 42% of those of the twelve airmen contained irrelevant description of appearance—what the symbols of the charts or the curves of the graphs looked like. In some cases this description was supplementary to reports of the meanings of information which had been fairly well understood. But, in other cases, the subject had been puzzled and baffled in the attempt to translate appearance into meaning. This is indicated by the following report by a student on the chart shown in Fig. 1: "This chart means very little to me. The income was represented in sums of 10/-. A figure of a man represented 100 persons, and there were other signs for other numbers of persons. The diagram was to represent Population, Income and Mortality rates. What the results were I could not gather, except that where there was more money there were fewer deaths." Or the subject might be so preoccupied with appearance as to pay no attention to meaning. Another student drew the symbols of the chart in Fig. 1 with the legends written against them, and then wrote: "I can remember the general picture" (which she also drew) "but did not properly grasp its meaning." Thus it appears that individuals sometimes visualized the charts clearly without translating the images into the information the chart was intended to convey. The tendency was due in part to the fact that the subjects

in this part of the experiments (see p. 146 (1)) were not certain as to whether they were expected to report 'appearance'. Some of the students were given a brief preliminary account of the information shown in the graphs and charts. This served to emphasize meaning rather than appearance, and in consequence only 21% of their reports included descriptions of appearance, as against 37% of the reports of the students who received no preliminary talk. It is, however, clear that in the absence of special instructions, too

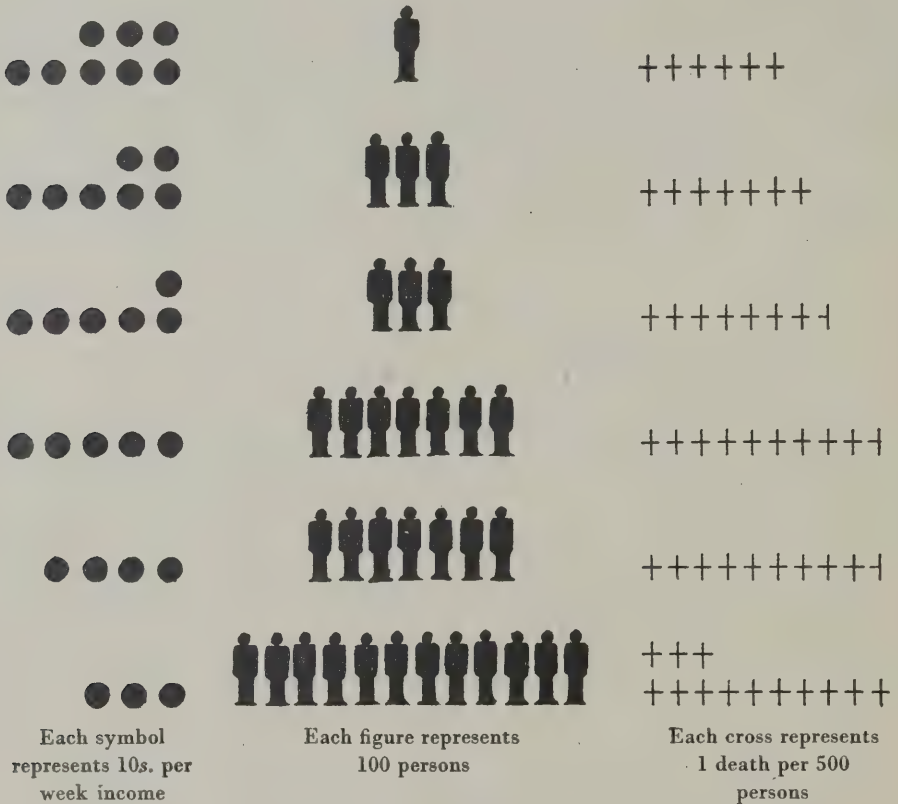


Fig. 1. Population, income and mortality.

much attention may be given to the 'appearance' of graphical types of presentation with a consequent deleterious effect upon the understanding of their meaning, especially by those who are not habituated to the interpretation of graphical material. It follows that in using such graphical material for instructional purposes, it is necessary to point out quite definitely how it is to be interpreted and what relation it bears to the data which are being presented.

(2) *Difficulties in perceiving the charts and graphs clearly*

Considering for a moment the perceptual characteristics of the graphs and charts as apart from those inherent in the meaning of the information they are intended to convey, certain points appear to be worthy of comment. On the positive side, there is no doubt

that relationships and directions of change in the magnitudes of quantities show up best in the graphs. Trends and points of intersection of the death rates from tuberculosis appear clearly in Fig. 2; the subjects' mean scores on these were: graph, 91 % correct answers; table of figures, 90 % correct; chart, 81 % correct. The relative magnitudes of populations are clearly demonstrated in Fig. 3 (Graph, 90 % correct; table of figures, 77 %; chart, 63 %). But, on the other hand, absolute magnitudes or actual figures of any kind were seldom noted accurately from graphs or charts, because the figures on the scales were rarely observed. Thus in Fig. 4, the exact point of reproduction of the population (1.0) did not stand out as clearly in the graph and chart as in the corresponding table of figures where the actual numbers were given (table of figures, 63 % correct; graph, 55 %; chart, 53 %).

Whenever a stress was required upon the *qualitative aspect* of the quantities studied,

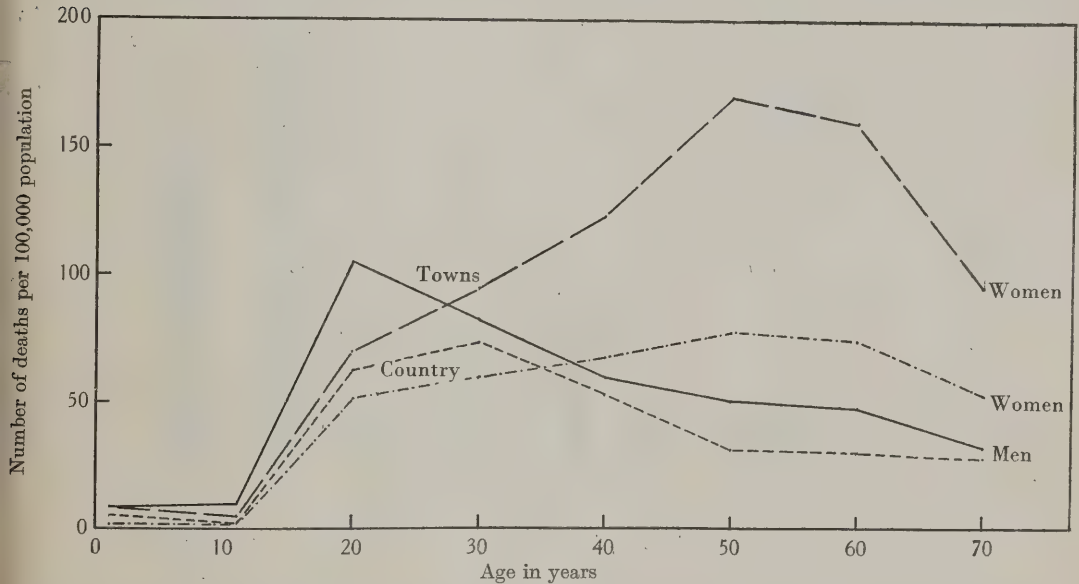


Fig. 2. Deaths at various ages from tuberculosis of the lungs of people living in towns and in the country.

the pictorial charts were valuable in so far as they could represent this aspect clearly and strikingly. But the stress on the qualitative aspect sometimes withdrew attention from the more generalized tendencies of the data. Trends and relative magnitudes did not appear very clearly in the pictorial charts. It was easy to note where magnitudes increased or decreased in parallel fashion from the top to the bottom of the chart. But it was much harder to see, as in Fig. 5, that some of the occupations increased their numbers, others decreased, others first increased and then decreased, and so on. (Table of figures, 13 % correct; graph, 12 %; chart, 11 %; the questions were badly answered with all three forms of presentation, partly because of the confusion of the data, and partly because these were the first questions asked.)

The probable conclusion is that for striking the attention and impressing simple and obvious data upon the comparatively uneducated, the charts may have considerable value. This conclusion is supported by some of the comments made: "The charts are more

impressive"; "the charts are more pictorial, they caught your eye"; "the charts create a picture in your mind." Moreover, Table 1 shows that, on the whole, the army group found the charts easiest and the tables of figures hardest; whereas the airmen, who were more intelligent and better educated, found the graphs rather easier than the charts. It

Table 1

	Percentage judging easiest			Percentage judging hardest		
	Charts	Graphs	Tables of figures	Charts	Graphs	Tables of figures
Army group	33	32	21	9	23	37
Airmen	35	43	13	17	17	37

seems probable that for anyone with some experience in the use of graphs,¹ the more complex type of data are better assimilated in this form, because they show general trends more readily—"you can see at a glance what's happened". On the other hand,

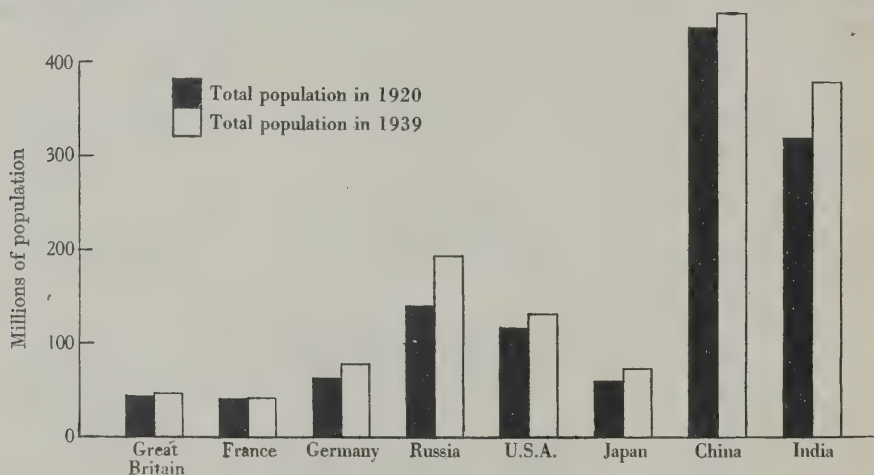


Fig. 3. Population changes in various countries.

the individual untrained in the use of graphical material may find graphs very puzzling at first and difficult to interpret. When the scores were compared for the three different ways of presenting the whole of the 'War Effort' data, no appreciable difference appeared. But the average scores for the first set of data presented to each man (Set A) were: charts, 47.3 % correct; tables of figures, 47.2 %; graphs, 42.3 %. This difference is not fully significant, but is larger than any other differences obtained between different methods of presentation of this material. Thus it seems possible that the graphs possessed for some men a 'shock effect' greater than that of the charts or tables of figures. Indeed there was one man who, on being presented first with the graphs, merely stared at them and then said that he couldn't make them out at all. And about half-a-dozen others were observed to look at one graph for the whole period given, and make no attempt to study the others; afterwards they attempted to answer the questions from such knowledge of

¹ Some of the army subjects had had experience in civilian or army life of the use of graphical material. They scored significantly higher than the rest of the army group, and were graded significantly higher on the general questions. But they also had higher scores on A.H. 4, so that it is not certain how much of their superiority can be attributed to previous experience, and how much to superior intelligence.

the subject as they already possessed. It may be concluded, therefore, that graphs are unsuitable for use by those who are unaccustomed to them, whereas charts, which bear some resemblance to pictures, 'put them off' less.

(3) *Difficulties in understanding terminology and concepts*

Difficulties also appeared if different numerical scales appeared on the same ordinate or abscissa of a graph. These were not entirely perceptual in nature; they resulted also from

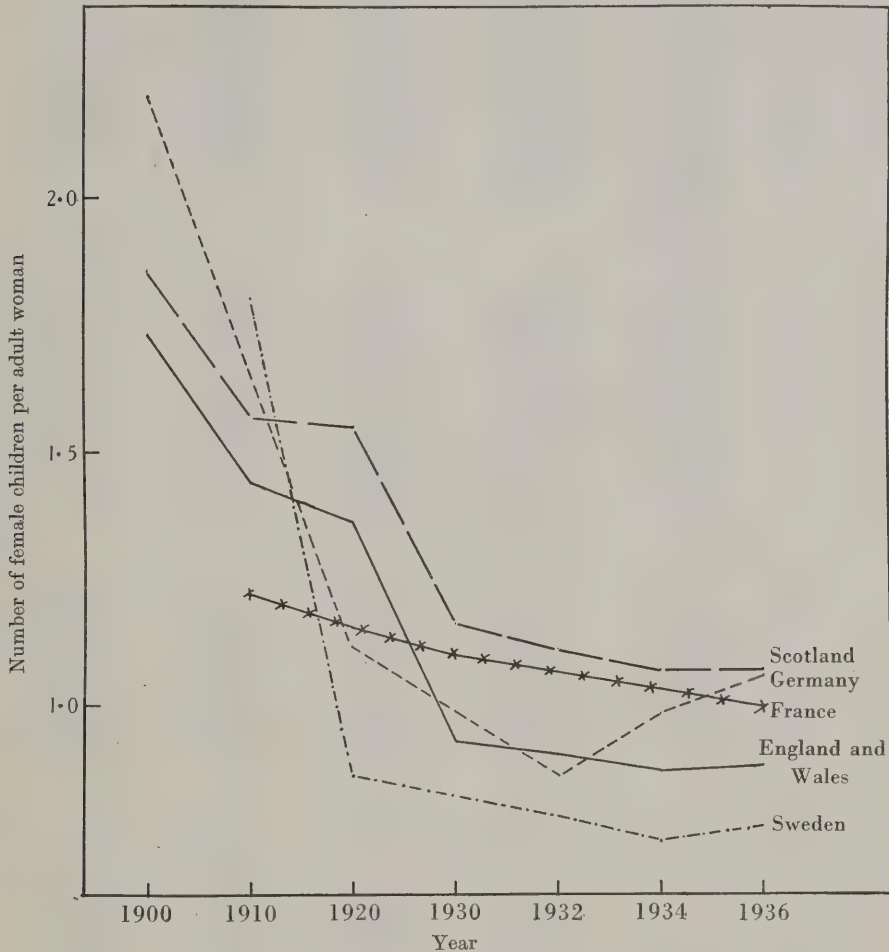
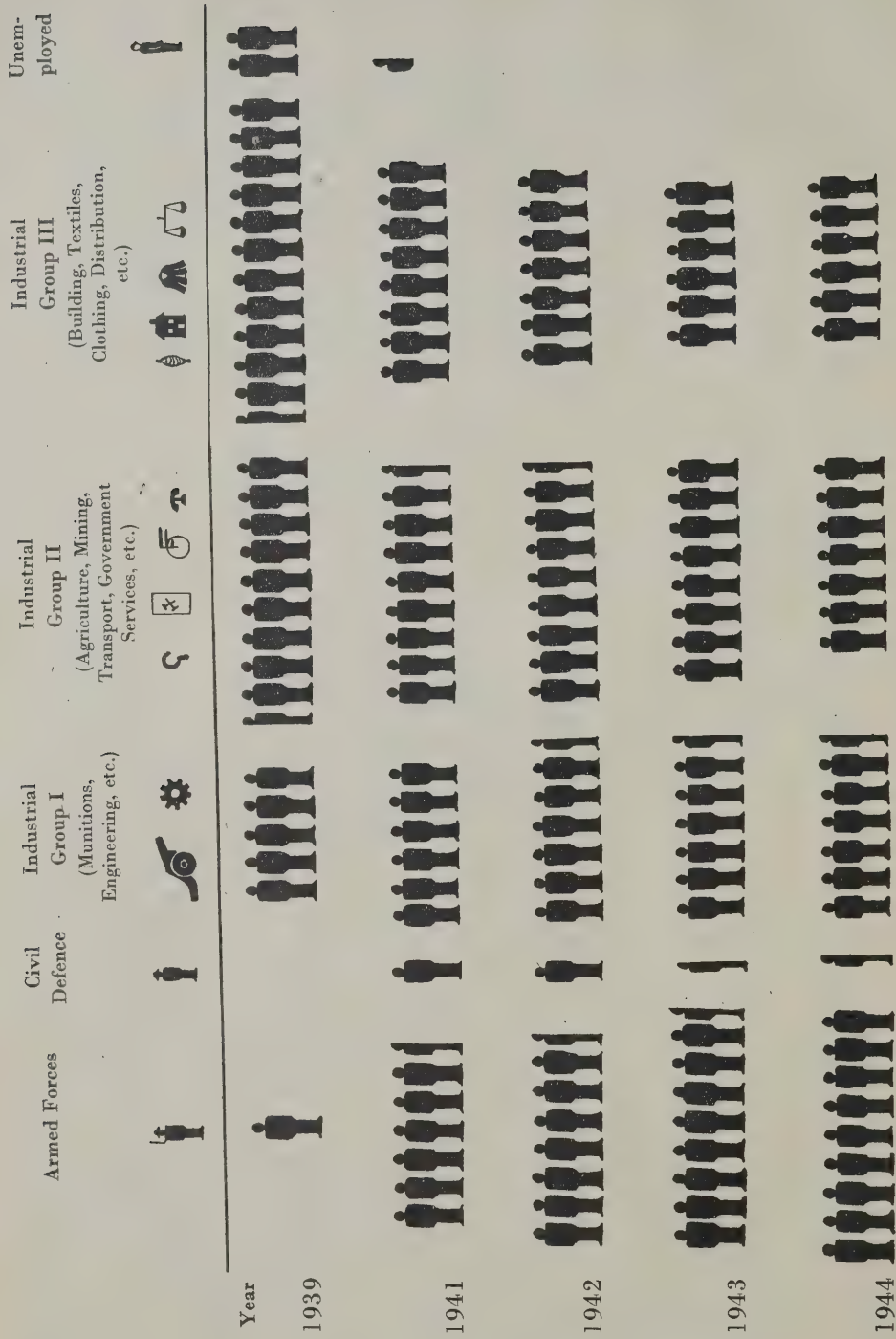


Fig. 4. The ability of the populations of various countries to reproduce themselves.

slowness in understanding the spatial representation of non-spatial quantities. Indeed, this was probably one of the root difficulties in dealing with all the graphs. The task became even more difficult when, as with the vital statistical data, it was not 'absolute' but 'relative' quantities which were usually represented. Even simple 'rates' and 'percentages' were confusing for those with little mathematical skill and training, because each one involved the relationship of two quantities, one of which was implicit—namely, the total, average, etc., on which the rate or percentage was calculated. In the use in



Each figure represents 500,000 men.

Fig. 5. Number of men aged 14-64 in Great Britain in various occupations.

vital statistics of terms such as 'birth rate', 'death rate', 'fertility', 'comparative mortality', it is not at all clear to anyone who has not studied the subject what totals, averages, etc., are implied as the second term of the relationship. Hence, for instance, the frequent failure which occurred to understand that a falling birth rate can exist at the same time as a rising population. When the relationship between two rates is involved, as in the right-hand set of curves in Fig. 6, the problem is harder; no one understood the implication of these curves, although 44 % of the airmen and students understood at least in part the meaning of the left-hand set of curves.

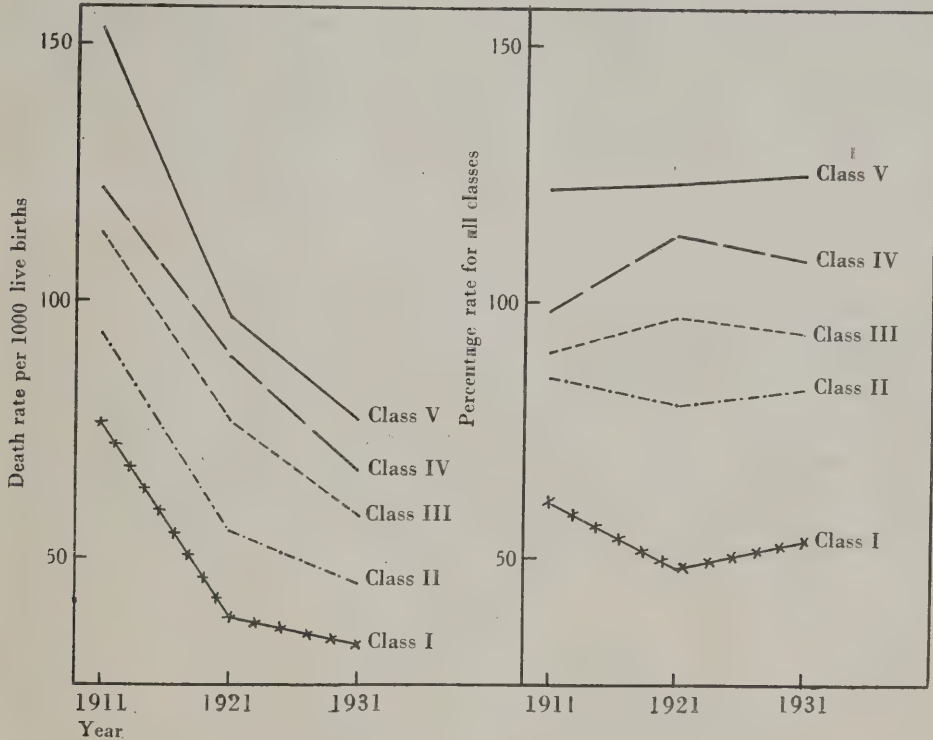


Fig. 6. Infant mortality in various social classes.

Social classes:

Class I—Professional workers.
Class II—Clerical workers.
Class III—Skilled workers.

Class IV—Semi-skilled workers.
Class V—Unskilled workers.

(4) *Difficulties in verbal formulation*

A point of fundamental importance in studying the ability to understand and remember information on matters of general social interest is to discover how far the apparent inability to state this information means that the individual is lacking in knowledge and understanding; and how far it is due to lack of facility in verbal expression.

There was evidence in this investigation that difficulties in expressing an idea which actually existed in the subject's mind did affect his answers to the questions. There was little doubt that the inadequacy of the verbal reports given on the first set of graphs and charts was due in part to the subject's difficulty in formulating his recollections. But it

was not possible to distinguish this effect from that produced by the lack of exact understanding as to what he was intended to report. Again, in answering the comparatively simple and straightforward questions about the 'War Effort' data, hesitations, repetition of clichés and awkward phrasing were frequent.

Even quite simple specific questions sometimes produced the same difficulty. To the question, "What has been the effect of the war on exports from this country?" answers such as the following were given: "Everything in case of munitions it hasn't increased to any great extent."

It seems probable, however, that the difficulties in verbal formulation went hand-in-hand with a lack of clarity in knowledge and ideas. The extreme vagueness of their answers was due in part to the vagueness of their ideas and in part to their difficulty in formulating them. It would perhaps be most correct to say that lack of clarity of ideas and lack of facility in verbal formulation react upon one another and produce a vicious circle; the ideas cannot become fully cleared up because, among other reasons, the individual has insufficient command of words to formulate them clearly even in his own mind.

(5) *Difficulties in generalization*

All the subjects were required to produce in addition to detailed answers or reports, general statements of some kind based upon the information contained in a number of graphs or charts. That is to say, some evidence was obtained as to their ability to reason inductively from particular facts towards a generalized conclusion.

The students and the twelve airmen were asked simply to state any general conclusions or general ideas arrived at in studying all the data presented to them. Table 2 shows the percentages of these conclusions which were graded A, B and C, and D. Thirty-six other airmen, after seeing a group of five charts or graphs or tables of figures, were asked

Table 2

	% of final statements graded		
	A	B, C	D
Airmen	10	50	40
Students	6	66	27

questions of a general nature such as: "What sort of changes have been taking place in the population of this country during the last fifty or a hundred years? Is the same type of change likely to continue to take place in future? Or if not, what other changes may be expected?" The answers were graded: A, 5 %; B and C, 70 %; D, 25 %. That is to say, only 5 % were fully adequate; yet over 70 % of the questions on individual graphs were correctly answered.

The army group were asked similar questions after seeing each set of four graphs or charts or tables of figures; one question was: "In what general respects have the occupations and ways of life of people in this country changed during the course of the war?" Here again the proportion of adequate answers was small; they were graded: A, 7 %; B and C, 58 %; D, 35 %.

In addition to completely inaccurate answers to these questions, there were two other main types of inadequate answer. The first was the more or less irrelevant, obviously resulting from a set of pre-conceived emotionally toned ideas; this will be discussed in

detail in the next section. The second type consisted of a list of specific facts, usually quite accurate in themselves, but unrelated in any form of generalization. Thus the following answer was produced to the question on change of occupation in war time:

At the beginning there was an increase in armed forces and Civil Defence, and also in munitions industries. About 1942 the increase continued from civilian to armed forces. The numbers employed in Civil Defence dropped off and the number of women in essential industries increased all the time, the biggest increase being up till 1943. They increased slowly afterwards.

Some even clearer examples appeared in the answers given by some of the students and airmen about particular graphs and charts. Thus from the simple graph shown in Fig. 7, 29 % of the students could not make the immediate generalization that the general effect of an unsatisfactory diet was to increase the incidence of certain defects in children. They gave instead long lists of specific defects and their frequency, first among children on an unsatisfactory diet, and then among those on a satisfactory diet, without apparently

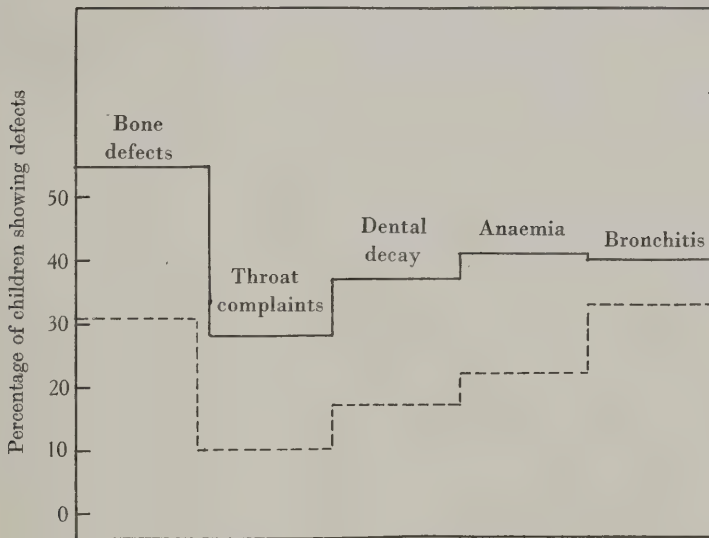


Fig. 7. Effect of diet on defects in children.

--- On satisfactory diet. — On unsatisfactory diet.

connecting the two. And of those airmen who were actually questioned: "What is the effect of an unsatisfactory diet on children's health in general?", one third either did not understand the question, or gave lists of specific defects rather than a general answer. But perhaps the most conspicuous defect in generalization was shown in respect to a chart illustrating the Family Allowance Scheme, which was taken with very slight modification from the official pamphlet on the Government's Social Insurance Scheme. Only 17 % of the students and 11 % of the airmen made a fully generalized statement such as: "The scale of payment is 5s. a week for every child after the first up to school leaving age." The remainder either made definite mistakes such as stating that the scale of allowance was 15s. a week, or that payments ceased after the fourth child; or wrote out laboriously: "Nothing for the first child, 5s. for the second, 10s. for the third", and so on.

Thus it may be concluded that, in order to supply the subjects with information of a general character, and to demonstrate how it may be gained inductively from particular

facts, both the general thesis and also the particular facts, and the relationship of the former to the latter, must be clearly traced. If this is not done explicitly, there will be a tendency either to fail to utilize the facts constructively because they remain isolated and unrelated to any general understanding of the problems at issue; or to make wild generalizations which ignore the facts.

(6) *The importance of a general background of knowledge and ideas*

Of even greater importance than the difficulties previously described were those which resulted from the lack of a general background of knowledge about social and economic questions, and of a context of ideas into which to fit the information presented. The set of data on vital statistics utilized in the earlier experiments was deliberately chosen for its probable unfamiliarity to the majority of the subjects, in order to avoid the effects of special knowledge and training; and in fact none of these subjects had made more than a cursory study of vital statistics. But in the absence of such special knowledge of the subject, a strong influence appeared to result from such general knowledge about social questions as should be produced by a good general education. The effects of education

Table 3

Score on A.H. 4, Part I	No. of answers graded			
	A	B, C	D	
Over 30	16	90	31	$\chi^2 (A, D) = 19.4$ $P < 0.0015$
Under 30	2	68	63	

were of course difficult to distinguish from those of intelligence, because generally the more intelligent were better educated, and the better educated more intelligent. Thus it was found that there was a correlation of 0.587 between the scores of thirty-six airmen with the graphical material and their scores on A.H. 4; and a correlation of 0.668 between the army group's numerical scores and their scores on A.H. 4, Part I (verbal). There was also an obvious relationship between the scores of the latter on A.H. 4 and their grades on the general questions, as shown in Table 3. But it also appeared that in some cases the effect of a secondary (grammar) school education was as important as, or even more important than, that of general intelligence. Table 4 shows that in one group of twelve airmen, those with secondary school education scored less well than those without it on A.H. 4; but the former did better with the graphical material than did the latter.

Table 4

	Mean score, A.H. 4	No. of reports graded					
		A	B, C	D	A	B, C, D	
Secondary education	70.75	11	43	9	11	52	$\chi^2 = 6.09, P = 0.014$
Primary education only	95.33	6	97	3	6	100	
Diff. 24.58							
$t = 3.57, P = 0.005$							

Again, it is possible to give specific instances of answers which were not only incorrect but wildly incorrect; where such mistakes could not have been made if the subject had had any general acquaintance with the matter. For instance, few people with any knowledge of the population situation or of public health in this country would say that the population of this country, judged on the basis of pre-war population statistics, was

likely to increase in future; or that people lived longer in the last century than they do nowadays; or that infant mortality has increased in recent years.

The data relating to the 'War Effort' were both easier to understand and also more familiar in content than the vital statistics. Thus if the grades of two groups of airmen on answers to general questions are compared for the two sets of data, as is done in Table 5, the performance on the former is seen to be significantly better than that on the latter. Nevertheless it was clear that many of the men in the army group were much at sea in understanding certain matters of information about the 'War Effort'. These were in some cases related to educational information, and in others to matters of general knowledge to civilians though apparently less familiar to service personnel. Among the former was the topic of exports and imports. It was clear that a number of these men did not really understand the difference between exports and imports, and data from export and import charts and graphs were often confused. Neither had they any knowledge of what

Table 5

	No. of answers graded		
	A	B, C	D
Vital statistics	5	76	27
'War Effort'	17	49	3

$$\chi^2 = 24.3, P < 0.001$$

goods are mostly imported into and exported from this country. Thus foodstuffs were mentioned among exports by 12 % of the men and coal among imports by 10 %. Obviously it is much harder for anyone to answer detailed questions about decline in imports and exports during the war if they have no knowledge as to the standard goods upon which our foreign trade is based.

The production of incorrect information in these cases was due to ignorance. In certain other cases it resulted from the swamping influence of incorrect ideas actuated by prejudices and emotional bias of some kind. This appeared particularly in the answers to the general questions. Fears and suspicions about the break-up of the home and the normal structure of working-class society appeared in answers to the question: "In what respects in general have the occupations and ways of life of people in this country changed during the course of the war, according to the information given in these charts?" Instead of answers which *were* based on the information given in the charts, etc., quite irrelevant statements were made, such as: "Changed for the worse, not for the better; war's no good to anyone; it's broken up homes; there's more drinking and smoking, and married women going with other men."

Another conspicuous trend of emotionally directed thought was indicated by a satisfaction that the civilian has had to suffer too in this war, and in particular that production and consumption of luxury goods has been cut down. It was interesting to note that the word "luxury" appeared in 27 % of the men's answers, although it was never used in the graphs, charts or tables of figures; and indeed no data were given which related to purely luxury goods apart from cars. This trend of thought is illustrated further in the following answer to the question "What light do these charts, etc., throw on the alteration of civilian habits during the war?": "Makes people realize there's a war on; not a soldier's war but a civilian's war as well."

Thus it appears that, on the one hand, people will tend to ignore facts when their ideas are mainly directed by emotionally toned opinions; and on the other hand in so far as

their knowledge of the problems at issue is vague and indeterminate, they will be more ready to be guided by irrational prejudices. As mentioned in the last section, only a course of general statements combined with a reasonable, but not excessive, amount of factual detail is likely to establish real understanding and to correct prejudice. The graphical presentation of the factual detail may be successful in so far as it is simple and clear. But it cannot state generalizations. Merely to accompany it by a brief introductory general statement, as was done in all except the earliest of these experiments, is not sufficient. It would probably be necessary to go backwards and forwards several times from generalization to details, ensuring by questioning that the connexion was fully established. How little many people can generalize accurately and reason inductively seems demonstrated fairly clearly by the results of these experiments. Moreover, it is not sufficient for the individual to be 'aware of' either a series of facts or a general proposition in a vague kind of way; he must have incorporated these into a stable, familiar and functioning system of ideas. A long and gradual process of building up such systems in a clearly and coherently related fashion will be necessary before they can direct his actions, counteract his emotionally toned prejudices, and assist him to acquire new information readily and accurately.

IV. SUMMARY

1. Two sets of numerical data, the first on vital statistics and the second taken from the Government White Paper on 'Statistics Relating to the War Effort', were presented graphically to 231 students, airmen and soldiers, who were required to recall the information given, either in their own words or by answers to questions.

2. The understanding and remembering of these data appeared to depend upon ability to: (a) interpret graphical material, i.e. to transform spatial representations of quantities into the corresponding abstract ideas of quantity; and, in some cases, into ideas of relative quantity such as 'rate'; (b) understand and use language efficiently; (c) generalize from particular sets of factual data to general statements or arguments; (d) think relevantly on these topics and not be swayed by preconceived and emotionally toned ideas and opinions.

3. In general, the subjects of these experiments did not appear to possess these abilities to a high degree, although those with greater intelligence and better education were superior to those with less intelligence and education. It seemed necessary for the development of these abilities to set up clear and well-organized and inter-related systems of ideas about matters of general information, e.g. on social questions, not necessarily related to particular practical tasks. Those who were lacking in such systems of ideas had great difficulty in acquiring any information of this kind, because there was nowhere into which it could be conveniently fitted; hence it tended to be ignored, forgotten, isolated or transformed to fit in with ideas and opinions which were of an emotional and irrational nature.

In conclusion, my acknowledgements are due to the Medical Research Council which made it possible for me to carry out this enquiry; and to Prof. Bartlett for permission to work in the Cambridge Psychological Laboratory. I wish to thank the Principal and tutors of the Training College, and those authorities in the Army and the R.A.F., who enabled me to obtain subjects for the experiments.

PERSONALITY STRUCTURE AND MEASUREMENT

II. THE DETERMINATION AND UTILITY OF TRAIT MODALITY

By R. B. CATTELL, *Harvard University*

I. *The classification of trait unities* (p. 159). II. *Misleading assumptions about the nature of modality distinctions* (pp. 159-161). III. *Extraction of formal modality characters inherent in current usage* (pp. 162-163). IV. *Operational indicators of modality* (pp. 163-166). V. *The formal recognition of incentives and complexities* (pp. 166-169). VI. *The utility of modality distinctions in providing 'conditional factors'* (pp. 169-173). VII. *Some unresolved issues* (pp. 173-174). VIII. *Summary* (p. 174). *References* (p. 174).

I. THE CLASSIFICATION OF TRAIT UNITIES

Psychologists, following common-sense usages, have found some convenience in distinguishing three classes or 'modalities' of traits: (1) Dynamic traits, e.g. dispositions, sentiments, neurotic symptoms, ergs⁽²⁾; (2) Temperament traits, e.g. general emotionality, surgency, perseveration, hyperthyroidism, personal tempo; (3) Abilities or cognitive traits, e.g. native general intelligence, acquired perceptual and executive skills. Perhaps the term 'modality' implies to some psychologists a more radical mode of difference than is constituted by these divisions; but for lack of any really suitable term, we must perforce use it in this way, at least temporarily. In the light of later discussion the expression 'conditional modality' or 'conditional variety' may well recommend itself as the most apt designation.

Distinctions of modality are drawn in practice with considerable confidence and unanimity. Yet it would puzzle the majority of psychologists to give an explicit definition of the way in which these distinctions are effected. The aim of the present paper is to discover the formal and operational distinctions of modality, to ask what utility modality classifications have, and, if possible, to reveal latent categories and latent predictive uses not yet obvious to common-sense and unselfconscious current practice.

Consideration of the modality of traits, in any systematic fashion, is rendered possible only upon the ground work of the preceding paper⁽⁷⁾, which investigated the means of discovering trait unities as such; for one cannot classify a population, the members of which have not first been themselves defined.

II. MISLEADING ASSUMPTIONS ABOUT THE NATURE OF MODALITY DISTINCTIONS

Certain current misapprehensions have to be removed before undertaking constructive examination. In the first place it cannot suffice to define temperament, as McDougall, in the wake of popular practice, has done, by calling it the inherited aspect of personality, or that aspect of mental life which is correlated with physiological conditions. For all mental life is correlated with physiological conditions and many traits besides temperamental ones are largely inherited, e.g. general intelligence or the dynamic manifestations of manic-depressive disorder. In any case, from the manner in which modality distinctions are made, one would expect them to depend on some more immediate, behavioural character of the trait rather than on facts about its origin or its remote associations only to be discovered by prolonged research. The particular nature-nurture ratio value, or the

physiological or anatomical associations which it possesses, are properties usually established long after the modality has somehow come to be commonly recognized from more immediate behavioural properties.

Another barrier to clearer understanding is the uncritical but very natural habit of assuming that any trait unity is of only one modality throughout. It seems to be overlooked that though a source trait or mathematical factor, as commonly obtained in most factor analyses, is a pattern of elements in only one modality, this result arises from the special selection of variables commonly made in factor analytic experiments. Without such selection by the experimenter a factor might well cover all three modalities; indeed the writer contends that this habit of pre-selection for one modality has been an unfortunate trend in the recent history of psychology. Surely it would be of great interest and utility to know what temperamental and other general personality trait elements usually go with, say, mathematical, verbal, practical and artistic abilities. Even the practical work of guidance is impoverished and impeded by this lack of knowledge of the persistent associations existing between abilities and personality features.

This mixed character of the common source trait seems likely to obtain with both what have been called (3) constitutional and environmental mould traits. A gene, the occurrence or absence of which causes the covariation of the parts which we recognize as belonging to a constitutional trait, is very likely, as far as our present knowledge of genetics goes, to affect simultaneously cognitive, temperamental and dynamic aspects of behaviour. For example, the Mendelian dominant responsible for Huntington's chorea obviously determines at least temperamental and ability modalities, and probably dynamic trait elements too.

Similarly a single environmental mould trait, e.g. the pattern of elements covarying in a factor produced by the varying impress of 'repressiveness of family discipline', on different individuals, may manifest, in the pattern of the die, dynamic, temperamental and ability trait elements. In short, a more catholic choice of variables in factor analysis, and a more adventurous use of the correlation method, would be expected, theoretically, to lead to factors of broader meaning and modality. That outcome, indeed, has already been demonstrated in practice (4, 5). This brings us to the preliminary conclusion, to be amplified later, that most of the unitary traits so far produced by factor analysis, which are characteristically only of one modality, are artificially truncated forms of patterns which are naturally wider, the truncation being accomplished by pre-selection of variables. For some purposes, as we shall see, the deliberate restriction of a trait to one modality of trait elements has special advantages; but the fact that some are and some are not restricted needs to be explicitly recognized and its consequences formulated.

The third and last of the treacherously misleading current assumptions is that any single manifestation of behaviour, as in a trait element used as a correlation variable,¹ *can* be solely of one modality. (This is different from the above-mentioned assumption that source trait patterns *are* typically of one modality. They *can* be.) The fallacy in this mode of thought may be recognized from several approaches, of which two may suffice here.

¹ To avoid tiresome circumlocutions we may speak at times of elements or patterns of *behaviour* when strictly the reference is to elements or patterns of *mental structure*—the mental structure which accounts for the episodic appearance of the observed behaviour in question. The trait or trait element is, of course, the mental structure rather than the behaviour from which it is inferred, but their mutual representativeness enables them to be used interchangeably, without confusion, in the present context.

First, in a factor analysis of say a dozen behaviour variables, the total variance of each narrow variable is usually accounted for not by one factor but by all or most of the factors present in the group of variables, some of which are of one modality, some of another. That is to say, the same measurement, e.g. speed of completing a task, is a measure partly of ability *A*, partly of ability *B*, partly of temperament trait *C*, partly of effort factor *D*, and so on. True, in measuring a particular source trait or factor, it is usual, in practice, to pick out two or three variables, the greater part of the variance of each of which is accounted for by the factor in question, and add together the scores on these, as if they were solely measures of one factor and one modality. For example, we make an 'intelligence test' measurement from adding the whole score on those few subtests in the battery with highest 'g' loading. Here it is *approximately* correct to consider certain variables as measures of one factor or one modality, but it is theoretically inadmissible to consider any piece of behaviour as a *pure* manifestation of one factor or modality. Strictly we should add only that part of the trait element score which contributes to the factor in question. A factor, as a *pattern* abstracted from many variables, may thus be of only one modality, as when we select tests to make an ability measurement, but the variables or trait elements themselves, corresponding to literal behaviour, cannot be so.

The fallacy of considering one modality to be purely represented by a certain piece of behaviour can be indicated also from first principles. Clinical psychologists have sometimes been led by the seeming logic of their observations to the curious conclusion that, since all behaviour is dynamic or purposive, there can be no traits that are not dynamic traits. It is indisputable that abilities and temperament traits only manifest themselves in the course of, and in subservience to, dynamic behaviour, namely as the 'how' and the 'when' of a behaviour sequence primarily directed to and determined by some final satisfaction. They are, in other words, abstractions from an observed natural sequence of behaviour, itself directed dynamically to a goal, and they cannot have an existence of their own apart from some piece of behaviour directed to a goal.

Yet a moment's reflexion will show that the dynamic trait, as distinct from the dynamic behaviour, is also an abstraction, an abstract pattern (i.e. relation) of behaviour elements determined by the question 'why?' or 'what for?' The dynamic trait consists in fact of a set of equivalence relationships. To state that such and such is a dynamic trait unity is to be able to point to a pattern of (a) dynamic equivalents, e.g. to say that getting into the limelight and sulking are both aspects of a single self-assertive erg, and (b) a sequence of intermediate goal responses on the path to some more fundamental goal, e.g. that pursuit behaviour by a hungry animal and the killing of its prey are part of the hunger drive. Thus (b) and (a) are as the warp and the weft in the woven texture of behaviour constituting a dynamic trait.

Any narrow sample of behaviour which we take as a measurable trait element, therefore, is at one and the same time something which forms part of several wholes and can be interpreted in several ways,¹ just as a certain coloured thread in a tapestry can contribute to several distinct but overlapping patterns.

¹ These alternative abstractions, and the way in which they stand out by virtue of the direction of the observer's curiosity or by intrinsic prominence as determining factors, may be illustrated by the example of an alleged murder. The legal enquirer is principally concerned to examine the act of shooting from the standpoint of its intention, i.e. of the dynamic goal sequence. A ballistics expert, if he finds that the shooting has been done at 400 yards, is principally impressed by the ability aspect. The psychiatrist, if the motivation has been shown to be inadequate and the ability unquestionable, in turn becomes interested in the temperamental quirk which causes a man to become so excitable on trivial provocation.

III. EXTRACTION OF FORMAL MODALITY CHARACTERS INHERENT IN CURRENT USAGE

Although a variable cannot be wholly and solely a measure of any unimodal factor trait—since it contributes to many—it is possible to find variables which are very substantially composed of factors of one modality. From the inspection of such variables, highly loaded with the modality he seeks to understand, the experimenter may hope to discover, in concentration, the essence by which the modality can be defined.

Now in considering the character of any measured psychological variable one might be tempted, in a spirit of logical analysis, to begin by asking: 'Is the peculiarity of the modal character of this measurement something intrinsic to the form of the measurement, i.e. to the units and manner of measurement, or does it lie in the design of the situation within which the measurement is taken?' A moment's reflexion, however, will show that they are part of a single whole, and that the measuring pattern only gets its meaning in relation to the situation. By analogy, in physics we have, for convenience, voltmeters and ammeters, but what is measured depends on instrument-in-relation-to-total-situation. Nevertheless it may be possible to point to those characters of the response measured, on the one hand, and those characters of the situations, on the other, which, in conjunction, favour a certain modality of measurement. Whether a measurement shall be of an ability, a temperament trait, or a dynamic trait is obviously, in general, decided both by certain characters of the response measurement form (e.g. speed, errors, acceleration, frequency of response, goodness of response, uniformity of response) and by certain characters of the situation. Alterations in either may affect the modality. There is no more antagonism or independence in these two approaches to the problem than there is in measuring a certain distance or by rotating the ruler to the map or aligning the map with the ruler.¹

However, for reasons shortly to be indicated, it seems better to approach the definition of modality by considering the different settings under which one and the same measurement acquires diverse new meanings rather than the different ways of framing the measurement of performance which give new meanings to performance in one and the same field. For, in the first place, we *cannot* get very much variation in the form of the performance measurement, and, in the second, there is in fact surprisingly little available knowledge, or even speculation, about the directions in which modality character could be influenced by systematic changes in measurement form, e.g. measuring frequency or acceleration or oscillation. In amplification of the first objective it is enough to point out that the great majority of psychological measurements, whether in the laboratory or the clinic, are either literally or in essence measurements of speed or errors (frequency being a form of speed measurement). What these measurements mean—abilities, drives or temperament traits—is decided by the nature of the situation in which they are measured.

Now it seems to the present writer that the field characters by which we implicitly distinguish modalities can be embodied in operational definitions as follows:

- (1) Dynamic traits are involved in those variables the measures of which change most in response to changes of incentive.
- (2) Abilities appear in those variables the measures of which respond most to alterations in the complexity of the path to a goal.

¹ For example, an ability may be measured by the total time for a certain performance, a temperament trait by the acceleration shown in that performance. Alternatively traits of both modalities may be measured by total time of performance, but under two different conditions of the field governing the performance.

(3) Temperament traits are most involved in those measures which alter least in response to any changes in the field.

A critic may be excused for having the momentary impression that this is an attempt to avoid the real task of definition. It seems at first merely to barter unknowns in psychological definition for equally evasive features of the field. Or it may appear a brazen tautology, presenting a circular definition wherein dynamic traits are defined by 'incentives' and abilities by 'difficulties' or 'complexities'. Actually the mere shifting of the crux of the definition from the quality of behaviour to the characters of the field would constitute a slight advance; for observers seem better able to agree on the latter than the former. But the definition advances beyond a tautology in two respects. First, we propose to define the field characters without leaning on the conventional assumption that they are immediately known—for, in fact, they are at present known only circularly, through knowing modality! Secondly, we do not say merely that a dynamic trait is that which is 'tied up with' an incentive: we set up an operational test for this relationship.

IV. OPERATIONAL INDICATORS OF MODALITY

Let us postpone until the next section the definition of the field characters and see how the relationship to the latter is to be established, assuming that they can be recognized. There appear to be two principal ways of doing this, though it is not yet possible to assert with insight that they are the only ways. They may best be illustrated by a concrete example, namely, the experiment of testing a population of rats on a series of mazes, the mazes having different degrees of complexity and offering different degrees of drive satisfaction.

By the first method we take any one maze at a time and alter, independently, in one experimental series, (a) its complexity and, in another, (b) its incentive, plotting the field changes against changes in score (time or errors). The latter may be expressed either as the mean for the group or the standard deviation of the group of measures. If, for instance, the complexity is increased steadily from a range in which it is altogether too simple to offer difficulty to the average rat to a level at which the maze cues are altogether too hard for solution, we should expect an ogive type of curve to appear, relating difficulty to mean time, as in Fig. 1. (If the s.d. is used we should expect it to be at a maximum in the middle range.) At the beginning, when the maze is very straightforward, all rats will run it with ease, and it will not differentiate their abilities. Differences in speed will represent differences in drive strength or temperamental tempo. At the upper extreme of complexity it will again cease to be a measure of ability, for its insightful solution will lie beyond the range of individual differences in ability, and the trial and error behaviour will become again expressive of drive strength or some temperamental energy. There will be, therefore, some middle range in which the change of the mean score (or deviation) with change of complexity will be greatest. The whole will form an ogive curve, the steep portion of which we may call the *ability discrimination range* and the two flatter portions of which may be called *ranges of ability irrelevance*. If the original complexity of this maze falls in the discovered ability discrimination range the variable *A*, i.e. the measures on this maze, can be considered largely an ability measure.

Next, keeping the maze complexity constant in one of the 'irrelevance ranges', preferably the lower one, we begin to vary continuously the incentive strength. Here,

similarly, we may expect to find a range of variability in which, in the given conditions of complexity, scores on this variable become largely a measure of a dynamic trait. (When mazes are easy and the incentive is sufficient, differences of speed reflect only motivation differences.) Abilities and dynamic trait variables are thus defined by changes in the field, without changing in any way the form of measurement.

It may be objected that even though we recognize what is, say, 'complexity', we cannot form a true scale of *equal increments* of complexity for use in Fig. 1. This is undoubtedly

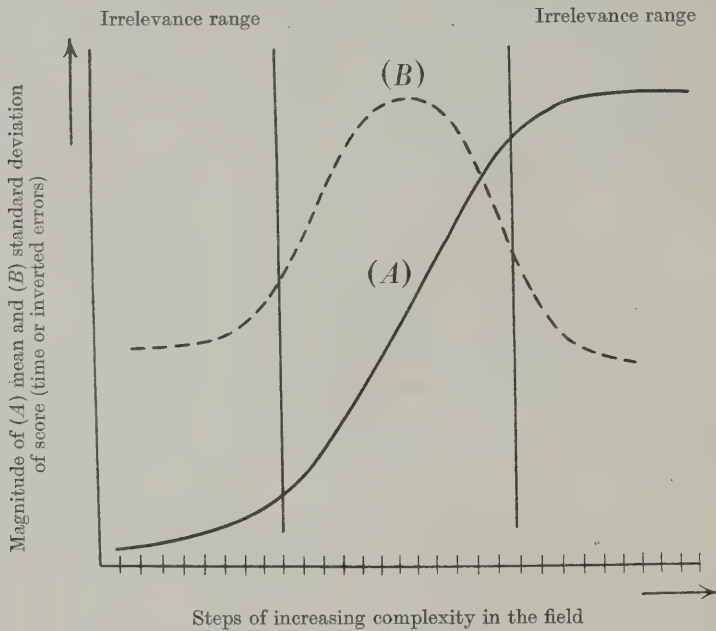


Fig. 1.

true, but Fig. 1 does not assume that the ordinate is a scale of equal intervals. It takes only a series of increasing complexities, at intervals as nearly equal as the experimenter can judge, and picks out *the particular intervals which produce big changes of score*.

The second method by which it is proposed that the predominant modality in any variable can be discovered is as follows. The mazes, measured in their original, unchanged form, are intercorrelated as 'tests' on a population of rats, and factorially analysed. The combinations of maze complexities and incentives are then altered, so that the incentive offered with maze A is now offered with maze B, or assorted according to some such systematic, cyclic re-coupling, and the intercorrelations recalculated. When both correlation matrices are factorially analysed, we may expect to find that certain factors have recognizably similar loading patterns attached to the complexities and other loadings which adhere to the incentives, 'following them around' no matter with which maze they are coupled. This permits us to recognize the factors whose loadings attach to specific difficulties as ability factors and to others as dynamic factors. From recognizing the patterns (factors) we can proceed to determine the single variables which most purely represent each type of measurement, by reason of their possessing the largest saturations with the factors (of recognized modality) in question.

Finally, the first method above could be employed within the framework of the second method to produce what is really a third independent method. For the range of complexity variation (in a particular variable) which produces the maximum change in mean performance is likely to produce the maximum scatter of individual measurements; and the maximum correlation between this variable and some other variable is likely to occur when the range of score in each is greatest. Consequently there will be some middle range of complexity variation, for each of two or more variables, at which the loadings of some common factor will reach a maximum (for these variables). It can then be concluded that the factor in question is an ability factor and that the variables, fixed in that complexity range, are largely measures of ability. This third method contrasts with the second in that the factor loadings are changed *in situ* by freely manipulating the field characters (one at a time) instead of interchanging fixed characters as in the second method.¹

Both of the above principles are implicitly admitted in, for example, intelligence test practice. In application of the first principle the psychometrist does not begin to give a test until he is sure that the motivation has shifted into the upper flat portion of the motivation-performance curve, the region of 'irrelevance', where the subject's attention is so adequate that slight differences of concentration are not enough to affect performance. On these grounds the general factor extracted from a collection of measurements under such conditions is considered an ability factor, not a dynamic or temperament factor; indeed these conditions are clinical prerequisites for an ability test. The implicit appeal to our first principle is seen also in intelligence test design. If the complexity of the tests is much increased or decreased relative to the ability of the population, the 'g' factor becomes much reduced, and the test fails as an intelligence test and is recognized to respond to other influences than abilities.

It is not so easy to point to any accepted unsystematic or accidental uses of the principle behind the second and third methods above. All factor analysts now recognize, of course, that the factorial composition of a set of tests alters with the population and the conditions, particularly as they affect complexity or incentive in relation to the abilities or interests of the given population⁽¹⁾. Woodrow⁽¹³⁾ in particular has discussed the observed changes in factors in the course of learning, as tasks become easier. It is through the consideration of preliminary findings in this region that the writer feels confidence in propounding the above methods as practicable.

In the general formulation propounded in this section, it will be observed that temperament modality is virtually defined by a process of elimination. Dynamic and ability

¹ With appropriate modifications these methods could be employed not only with person-test correlations—the plane *OA*, *OB* of the Covariation Chart (7)—but also through the other available planes. For example, one could present a single maze to a number of animal subjects on a number of occasions, one aspect of the maze, say the incentive, being continuously varied through the series of occasions. On analysing factorially the correlations between occasions the factor with the larger loadings on the middle range of occasions could be taken as a dynamic factor. Again, one could give all mazes to one subject on a series of occasions, varying, say, the incentive with different occasions. The factor which increased with greater variation of the incentive (the complexity being constant) would be the factor (from correlation of the mazes) of a dynamic character. Unfortunately it would not be entirely safe to generalize that the variable which proved to be the most saturated measure of a dynamic trait in this one individual would be so in all individuals, i.e. the measurement in this case is only of a *unique* trait.

These variants of the second and third methods could be matched by longitudinal variants of the first method. Therein the 'ratio of mean score change to mean situation change' would be worked out for a single individual on many occasions of testing, instead of between many individuals on one occasion.

modalities are defined by reference to some positive character of the field: temperament is defined as that which responds least to changes in these definitive characters of the field. Naturally the measurements suspected of being those of temperament may be scrutinized for least change in any suitable type of covariation situation, e.g. one may look for measures having very slight differences (*a*) between people or groups of people known to have very different dynamic and cognitive trait endowments, or one may look for the measures which change least in score, (*b*) on different occasions—occasions where differing incentives and complexities cause different degrees of provocation of dynamic and cognitive traits.

V. THE FORMAL RECOGNITION OF INCENTIVES AND COMPLEXITIES

It has been suggested above that the mere shifting of the emphasis in modality definition, so as to put the weight on field characters instead of on behaviour qualities and measurement forms, is in practice an appreciable advance. For, in studies of rats, for example, everyone knows that a whiff of cheese is the incentive, or an extra turn in the maze a complexity; while in studies on human beings the experimenter's own introspections can be taken confidently as a guide to what is incentive and what complexity. Nevertheless, the analytically minded reader will not be deceived by this confidence of practice, and will probably agree that our scientific task is not complete until we have given some operational means of recognizing what things in the field have the character of 'complexities' and what features have the character of 'incentives'. Even in practice, guided by the sharpest intuition, the distinction is sometimes difficult. When a boy sets out to solve a wire puzzle or a young man to woo and win a coy young woman, it would be hard to say which features of the stimulus are incentives and which difficulties. Or again, in child-guidance practice, it is often difficult to decide whether the individual's failure, e.g. in socialization, is due to lack of some social ability or to lack of interest. To find objective methods of making *modality distinctions*, in short, is at times as practically difficult as it is practically important.

Now the process of recognizing that part of a situation—that goal or goal symbol—which constitutes the *incentive* proves to be essentially the same as the method of disentangling the elements of a dynamic trait. This methodological problem has been discussed fully elsewhere⁽³⁾. There it was shown that the ramifications of a dynamic trait can be explored and mapped by most covariation designs but principally by (*a*) cross-sectional, 'static' correlation studies, in which behaviour elements sharing a common goal are indicated by a common positive factor, while behaviour 'equivalents' are indicated by bipolar factors weighting the equivalents with loadings of opposite sign; and (*b*) longitudinal studies, using 'occasions' either in a group or an individual (intra-individual correlation of occasions) which reveal the course of 'teleological causation'¹ in a series

¹ Nowhere in the present writer's theory is the expression 'teleological causation' used with any implications in systematic philosophy, but only as a purely descriptive concept. Order can be most readily introduced into psychology, in its early stages of analysis, by saying that behaviour is 'caused' by a goal which follows it. It is not difficult to suggest ways in which this initial teleological causation eventually can be analysed into 'physical' causation; but psychologists would be more truthful, if less 'scientific', if they admitted that the simplest prediction at the descriptive level is possible by 'teleological causation'. In establishing a connexion of physical causation we seek, in the observed events, an invariable sequence: *A* is the cause of *B* if it—and it alone—invariably is among the events preceding *B*. Conversely teleological causation is proved by observations that *A* and *A* alone invariably follows *B*.

of behaviour manifestations. It is the second of these which is most apposite to the problem of identifying incentives. Let us therefore examine its working in more detail. We shall begin by assuming that the motivation (and therefore the vigour and intensity) of behaviour in any situation is determined by the need for some final goal satisfaction (or simultaneously by several goals) which may be called the 'final incentive' (and the attainment of which is indicated by the cessation of such behaviour). One can pick out also a series of intermediate 'incentive symbols' which the organism by learning or innate endowment has come to look upon as encouraging signals and as paths to the final goal. The task of measuring a dynamic trait begins with the preliminary 'dissecting out' of the behaviour elements which are largely determined by a common final goal and which are related in a network of subsidiary and equivalent elements. And the most important aspect of all this dissection, especially for labelling and recognizing the trait, is the discovery of the final goal which links all together. However, the task of recognizing the final incentive is the same as that of tracing dynamic subsidiaries for the trail of subsidiaries leads up to the final goal. Of course, one can, if necessary, pursue the inquiry only up to some convenient *subsidiary* goal or goal symbol when that suffices for the practical problem at hand. For example, a man may be avaricious both because of some ultimate goal of security and some ultimate goal of ambition, but for some purposes the making of money may be considered a goal in itself, and attention is then given only to the subsidiary activities leading up to, and concluding at, this goal. Most dynamic traits have to be picked out from a network of converging and diverging chains of subsidiaries, ultimately converging on the biological goals of the propensities or ergs(2).

The general experimental problem in all investigation of dynamic traits is that of picking out, from a large random collection of field-behaviour¹ variables, (a) those which are tied up in any one single subsidiary or equivalent plexus, as distinct from others which have no such dynamic connexions, and in (b) the more final goal from the more subsidiary, intermediate incentives within this plexus. The first has just been dealt with: it is the usual covariation exploration of dynamic connexions. The second is best considered through an example. One takes either a single subject or a group of subjects under the same regimen and measures them on two situations at a time, the situations being repeated with some daily change on a long series of days to create two correlation series. The daily situations encompassed in the correlations can be increased from two up to any number, giving a correlation matrix which can be factorially analysed from all possible pairings for correlation of the occasions of that number.

Consider a boy measured each school day on (1) attentiveness in last class of morning

¹ Since some confusion may be provoked by our tacit assumption that a behavioural stimulus and the associated behaviour can be considered for purposes of measurement as one variable or item, it seems necessary to digress to deal with this more explicitly. What we actually measure, in the variables which we use, is behaviour. But each element of behaviour is tied up with a piece of environment, as what may be loosely called a response. Even the final goal behaviour is a 'response'. We normally judge how hungry an animal is by the vigour with which it eats when it finally reaches food. We also judge the power of an intermediate incentive or goal signal by the vigour of the immediate response to that stimulus. In the above example, money-making is considered an intermediate incentive. The strength of this intermediate incentive is measured by the vigour of money-making activity. Sometimes the behaviour is itself the stimulus to further behaviour.

In short a behaviour response can always be so closely tied with an environmental feature (an 'incentive') that we can speak indifferently of correlating variables of behavioural performance or of associated field features. Thus we speak of correlating test situations, but actually we correlate the responses made to the various test situations.

school, (2) intelligence test performance earlier in the morning, (3) time taken to cycle home to lunch, (4) amount of lunch eaten (or speed of eating), (5) enthusiasm at an afternoon football game. We are likely to find, in such a longitudinal study that (3) and (4) correlate positively and (1) negatively through their inclusion in a single factor, proving that they belong to a single chain of subsidiaries. That is to say, on days when the boy is unusually hungry he will eat a lot of lunch, cycle home unusually quickly and be rather inattentive at the last class of morning school. Variables (1) and (3) might also be involved in another factor with (5), showing that the afternoon game is also instrumental in quickening the ride home.¹

Now the choice of the variable which may be considered the final incentive or goal (or the most final incentive or goal among the variables sampled) in the sequence of subsidiaries concerned, might at first seem possible on a very simple basis. In most examples which spring to mind the final goal is simply the *last* (temporally) in the series of correlated variables. But this is largely because we know enough, on common-sense grounds, to make it the last in our sample of events. However, even if we chose our section of variables more blindly, in a stream of ill-defined motives and incentive symbols, it might seem that the goal would still be the last *correlated* item. A little further consideration will throw doubt on this. Reaction to food immediately after having eaten is as much affected by the size of the meal as was the behaviour stimulated by the prospect of so large a meal, i.e. an event *after* the goal may correlate as highly, though negatively, in any longitudinal study.

Observation without experiment and statistical treatment is not enough to permit a confident final formulation here; but the present writer would venture the theory that the final goal is revealed by its having the highest saturation, among all variables, in the factor running through the chain of subsidiaries or dynamic plexus. For, the greater distance in time between the goal and the lesser incentives (compared with the most final incentive), as well as the existence of many 'chance' influences, and the imperfect symbolizations of the goal by the lesser incentive symbols, would uniformly work to attenuate the correlations of these lesser incentive variables with the final goal and with one another. Furthermore, unless it happens that all drives get satisfied at the same time in some one combined manifestation of final goal behaviour, the variance of any one of the final goal behaviour measurements (i.e. those associated with one drive) would be accounted for largely by one factor, whereas any other variables of comparable dynamic strength would tend to have their variance accounted for by the summation of quite a number of diverse factors. For example, the energy with which a man works in his vegetable garden is a function of several final incentives, of which hunger is only one, but the energy with which he hastens to a meal is more largely a function of hunger alone.

The final incentive, therefore, may seemingly best be distinguished by a combination of criteria: (a) by being the last correlated variable, (b) by being followed predominantly by variables with negative saturations, and (c) by having the highest correlation in the given dynamic factor pattern; but the most reliable of these is likely to be the last mentioned.

So much for picking out the variables locating stimuli which are predominantly

¹ An alternative mathematical arrangement suitable for some situations would be to take another dimension of the Covariation Chart (7) and correlate a long series of events on only two occasions. A perfect correlation (apart from errors) would indicate that all the items chosen belong to the same dynamic pattern. This could be used where one has intuitions about the pattern, and one would work, by rejecting some items and adding others, to raise the correlation to a level suitable for some particular purpose.

incentives. Regarding the other primary environmental features—complexities—our analysis suggests that they can be identified only by exclusion. In short, those changes in the situation which produce changed performance, but which are not identifiable as incentives, are complexities. It may be found, for example, that the time taken by a rat to choose between two alternative signals introduced in a maze correlates very little with any incentive items or with changes in the final goal strength, i.e. it falls into no dynamic trait correlation pattern tied to an incentive. If an alteration in this feature of the field produces an alteration of total time, and the altered new feature also is clear of dynamic involvement, we have produced an alteration in complexity.

This may seem a broad, almost sweeping definition; but there is no reason to believe that the environment can contain anything else besides goals (and their anticipatory signals) and difficulties. Any narrower definition of complexity would certainly not include all that comes in the traditional population of abilities, general and specific. However, this bewilderingly broad realm should be readily susceptible to subdivision, for from this point it is comparatively simple to pick out the conditions restricting the following ability sub-groups: (1) perceptual-discriminatory abilities, (2) executive-motor abilities and (3) the group which we call memorizing abilities and which are defined as temporal changes measured in the abilities already in the other categories.

The definition of incentives and complexities enables dynamic and ability traits respectively to be recognized, and from these, as indicated above, temperament traits are defined by exclusion.

VI. THE UTILITY OF MODALITY DISTINCTIONS IN PROVIDING 'CONDITIONAL FACTORS'

The above formal distinctions can now be examined (*a*) concerning their effectiveness in solving those difficulties which sometimes arise in practice in attempting to decide modality; and (*b*) in an attempt to throw light on the question of the actual utility of making modality distinctions.

In practice perhaps the most frequent difficulties arise in distinguishing temperament traits, both from abilities and from dynamic traits. Since both temperament traits and abilities measure the 'how' of a piece of goal-directed behaviour, it is not surprising that some alleged ability measures, fashioned in the belief that abilities are best defined as those traits which make an organism *able to reach* its goal, turn out to measure also temperament traits. For example, there are many tests in which temperamental quickness of response contributes to the ability score.¹ An interesting topical instance of this uncertainty about temperament factors is the 'F' factor of fluency of association, for years regarded by most English psychologists, including the present writer, as a temperament factor⁽⁶⁾, and recently reappearing in Thurstone's ability analyses as a 'W' factor of word-fluency ability⁽¹²⁾. Again, this common 'how' character led the present writer in an earlier approach⁽³⁾ to classify ability and temperament factors together as 'constitutional economic' or 'constitutional non-dynamic' factors. The practicability of this classification testifies to some real kinship of these modalities.

¹ The confusion in practice extends even to dynamic traits. Success in an occupation may be contributed to by the possession of those (dynamic) sentiments which make a good character. If abilities are falsely defined as what leads to success, character becomes an ability. This approach, moreover, makes abilities depend too much on the goal, for that which is an ability in one purpose is a disability in another. Thus the man of good character has a disability if he takes up a criminal career.

That the 'how' character of goal-directed behaviour, in the sense of 'how successful', is not strictly what we mean by 'ability', needs to be demonstrated more fully. For this definition would not only include many temperament traits, as indicated above, but also many purely physical measurements, e.g. the body weight would be an 'ability' in boxing. Moreover, as the introductory discussion shows, it would lead to many characteristics described as abilities in one situation being described as disabilities in another; for many temperamental and physical characters, e.g. nervous sensitivity or stature, would behave in this way.¹ It would become impossible to define an ability without defining a goal at the same time, and confusion would arise from the different labellings—now as an ability, now as a disability—of the same tendency. It is not at all certain—one awaits the verdict of experiment with interest—that the definition of ability in the more basic way here advocated would entirely prevent some skills appearing as abilities at one time and disabilities at another; but it would probably reduce such instances to curiosities. This being so the new definition would bring one additional, ancillary (but not infallible) criterion to the distinction of abilities: that no matter how wide the variety of variables (measured positively for success) included in a matrix, the loadings of an *ability* will be largely positive, while those of a temperament trait are likely to be, with approximately equal frequency, positive and negative.

There are some further implicit current definitions of ability which seem equally to be half truths and tend therefore to be eventually misleading, ending similarly in the confusion of abilities with temperament traits, but also in other confusions. For example, there is the notion that though both abilities and temperament traits are 'interchangeable', i.e. employable in the service of or in association with a great variety of dynamic traits, abilities can be employed at will, whereas temperament traits cannot be consciously controlled. True, a schoolboy can deliberately give an impression of poorer arithmetical ability than he actually possesses, but he does so by refusing the goal—that of excelling. That he cannot hide a temperament trait so easily in a temperament test is due, in the first place, to his not knowing—as he does in the ability test—by what signs the examiner is estimating temperament. He could score high or low at will on a speed test, a fluency test, or a perseveration test, if he knew in which direction this would influence the interpretation of temperament. Secondly, he faces the more systematic difficulty that temperament traits tend to show themselves in almost *all* dynamic traits, not merely in those operating in the field of a given complexity. It would show as much in the subject's accepting the given goal of the test as if he set himself some non-cooperative goal: indeed, he would have practically to refuse to manifest *any* kind of behaviour in order to withhold demonstrations of his temperament characteristics.

The alternative possible confusion of temperament traits—namely, with dynamic traits—arises from such facts as that some kinds of temperamental excitability, ardour, emotionality or rapidity of acceleration of performance can be mistaken for the strength of the drive itself. The proof that mistakes can occur in practice is shown by one of the classical temperaments—the choleric—being what we should certainly nowadays call a disposition (pugnacity) and therefore a dynamic trait. Again, some writers describing extraversion or surgency of temperament refer to the sociability there manifested as if it

¹ The present misconception about abilities has led to the popular paradox embodied in the French aphorism that a man may have 'the defects of his qualities'.

were a dynamic trait in its own right, rather than a permissive manifestation—through temperament conditions—of a dynamic trait equally present in desurgent temperaments. The simple test possible here is obviously to ask, ‘Is the behaviour interchangeable, manifesting itself in all dynamic traits?’

Only if the answer is affirmative can the trait be a temperament trait. This test would seem to leave only one trait open to dispute—a somewhat academic dispute—namely, that if *all* dynamic traits are very strong we should be tempted to call this a general trait of temperamental ‘energy’ rather than a dynamic trait. But this at least agrees with general custom and may yet prove to be the most correct way of regarding such a trait.

Finally, in the light of the above analysis, it is possible to turn to the problem of why modality distinctions make such an appeal to psychologists. One finds, strangely enough, that the utility of attaching modality labels is by no means obvious, nor does it seem ever to have been explicitly stated by those who frequently use them.

It will be recalled that any factor established in a purely random sample of personality variables is likely to include dynamic, temperament and ability manifestations within its pattern, no matter whether it is established on static, incremental, intra-individual or other correlation data (7). This motley representation is equally prone to show itself in *constitutional factors*, e.g. in the syndrome of dynamic elation, temperamental quickness and imaginative ability in the manic factor, as in *environmental mould traits*, e.g. in the dynamic sense of duty, the temperamental imperturbability and the ability to handle a rifle which, let us suppose, correlate in the infantryman.

The factors obtained in most test and experimental situations, however, differ from these naturalistic, ‘holistic’ factors in that through either (a) deliberate restriction of the choice of variables, or (b) maintenance of special conditions during measurement (e.g. approximately equal motivation in ability tests), they remain restricted to one modality. They are artificially truncated factors, obtained either by refusing to take cognizance of other parts of the pattern which would appear if testing were extended to further variables, or by maintaining artificial conditions. These factors have, as it were, ‘squeezed out’ some of the variables which would normally be included in the pattern. It may therefore be most satisfactory, since their restricted modality is not necessary and inherent, to describe these trait forms as having *conditional modality* and to refer to each as a *conditional factor* or conditional source trait.

One needs to bear in mind that when we speak of an individual having personality traits of a given strength, we refer to the mean strength of responses on many occasions. We also assume that the incentives and complexities in the individual’s environment remain a tolerably constant framework and one which is approximately the same as that operating for all people. Response variables measured (or, as is generally the case with personality traits, rated) in such general conditions lead to what we have called ‘holistic’ factors, in which variables primarily dynamic and others primarily cognitive may frequently be positively correlated, and, indeed, highly loaded with the same factor. A conditional modality measurement breaks up these correlations by imposing ‘artificial’ conditions in which either the normal incentives or the complexities of the subject’s constant total environment are manipulated for a special occasion and shifted into the ‘irrelevance range’, causing the loadings in these variables to vanish, as just indicated.

Of what use to the psychologist are these truncated factors, survivors of special conditions? One must conclude, following our general theory, that it would be pointless to deal with such conditional factor unities *unless the conditions imposed in experiment also occur so frequently in recurring real life situations that these unities will be apt predictors in many situations.*

Scrutiny will quickly reveal that seemingly 'special' conditions are somehow far more prevalent than one would expect. For instance, we admit students to certain professions by 'ability' examinations, under special examination room conditions in which all are keyed to the upper limits of effort, i.e. are in a dynamic incentive irrelevance range. But this is approximately true also in after life, for the characters of these people are such that extremely few fail for lack of seriousness about their profession or by reason of using their skills for improper purposes. If this were not so it would be necessary to examine also on dynamic personality traits.

Similarly the psychotherapist dealing with neurotic adults generally neglects measuring abilities and concentrates on personality variables of a dynamic kind. He assumes that differences in cognitive ability are rarely very influential in causing neurotic states. He can concentrate on dynamic traits because in life also, for most people, and as far as emotional difficulties are concerned, complexities lie in an irrelevance range. Using traits of conditional modality, rather than 'holistic' traits, is thus justified as an economy, as a dropping out of encumbrances known to be irrelevant from the beginning in certain common types of problem.

Another aspect of this same gain may be seen most clearly from the consideration of what may be called 'interchangeability' of traits. Naturally, any one source trait (factor) may operate in combination with any one of all others: that is the meaning of independent factors. We may, therefore, find variables constituted almost exclusively of a certain factor *A*, others in which *A* acts with *B* and others in which many factors operate together. A certain temperament factor, for example, may reveal itself equally in the company of an individual's pugnacity or his gregariousness. An ability, such as intelligence, may operate practically alone, or with other ability factors such as mechanical aptitude, or in the service of aggression or in the service of curiosity and fear.

Are any special limits to this interchangeability of conditional factors involved, however, in the special instance when a dynamic trait element (in a conditional factor) would be tied up with a particular ability (from some other conditional factor) in some large single 'holistic' pattern, if 'holistic' factors were sought? Let us suppose, for a moment, that drawing ability and irascibility correlate in a single 'holistic' factor. Obviously any individual highly endowed with this factor can still use his drawing ability in the service of humour or any other dynamic trait. But on an average he will use it more in the service of pugnacity; for the correlations on which the 'holistic' factor is based represent people's responses in the normal framework of existing environmental provocations (incentives) and this individual shows greater than average responses whenever a stimulus to pugnacity exists. Nevertheless, in general, a stimulus to pugnacity will not occur when drawing ability is measured, and if it does we can deliberately set up another special variable which rules pugnacity out of the picture and introduces the dynamic aspect of some other factor (substituting special incentive conditions for those which commonly obtain). The practice of thus freely employing factors interchangeably in any combination, when they are conditional restricted factors of one modality, is a reflexion of our almost

universal ability to set up special incentives and to heighten or reduce complexities. And since we very frequently want to predict in situations which we have thus manipulated, the restricted conditional modality trait is very valuable.

A second gain possible from using the restricted conditional factors might arise if the primary modality characters by which we believe modality to be defined are associated with further, secondary characters of importance. For example, if all temperament traits proved to be highly inherited, or closely connected with blood chemistry, etc., the separation based on the primary characters would prove of use in a great number of other situations. Some such differences seem to loom in the picture in the judgement of most psychologists. Dynamic traits, for example, are commonly less permanent than temperament or even ability traits. Consequently the pattern of a 'holistic' factor might often become quickly obsolete and one would predict more accurately and confidently from factors which assume less wide influence. For example, in the above supposed 'infantryman' environmental mould pattern, the ability to shoot acquired in soldiering might be used at some later time in other patterns than the dynamic sentiment of patriotism and duty (quite apart from the normal 'interchangeableness' discussed under the first utility above).

VII. SOME UNRESOLVED ISSUES

The present analysis makes no claim to have reached final clarification of all the issues it has raised. Outstanding among the untidy loose ends is the question as to whether there are any other conditions which might be held constant in experimental measurement and in real life and yield thereby advantageous conditional modalities extra to the three we traditionally use.

One can conceive of equalizing, or shifting to an irrelevance range, such influences as amount of previous experience, time since some previous exercise, age, heredity, fatigue—particularly in traits for industrial prediction—sense organ, perceptual as distinct from executive conditions, and so on. Our exploration has to be terminated here, but, at a glance, it seems that nothing is distinguished by these as fundamental or as widely useful as in the existing modalities. Mainly these conditions point to further possible divisions within the ability modality, notably between discriminative and executive (motor) abilities.

Introducing the condition of a constant time interval between measurement of performance—thereby measuring retentivity or plasticity—would seem to constitute a special form practically as radical as any in existing modalities. If this retentivity should prove to have little relation to complexity or incentive it might be advantageous to think of it as a fourth modality rather than as a form of ability.

Another question concerns the possibility of generalizing about the actual nature of the measurement in diverse modalities. Modality, we have contended, is best defined by the nature of the field, when the measurements are uniform, but if in many psychometric situations the field is very similar, then each modality might come to be measured by some characteristic pattern of measurements. For instance, many temperament measurements, e.g. of perseveration or oscillation, are in (temporal) ratio form, expressing acceleration or change. This may be possible because the temperament modality is fundamentally most unlike the others. The purpose of the ratio is clearly to cancel out the direct influence of any drive or ability by introducing it into both measurements. Collation of ability

and dynamic trait measurements in men and animals might indicate some trend in each, even though the majority deal simply with time and errors.

VIII. SUMMARY -

1. Personality factors established without any systematic pre-selection of the variables to be correlated, and without the imposition of special conditions (other than constancy, precision, etc.) in the field during measurement, may be called 'holistic' source traits; for their patterns will include, normally, elements of all possible modality character. That is to say: dynamic, temperamental and ability traits are likely to be correlated in any one syndrome. Working on an intuitive basis, psychologists have, however, arrived at patterns considered exclusively of one modality, and have used them widely in prediction.

2. Two methods—one for single variables and one for factor patterns—are propounded whereby traits (of dynamic modality) which are predominantly responses to incentives can be operationally distinguished from those (of ability modality) which measure response to complexities.

3. To complete the methodology it is shown that operational criteria can be developed to discover those field characters which are incentives (or 'goals') and those which are complexities. This finally permits the recognition of dynamic traits and of abilities, whence temperament traits are defined by exclusion.

4. The practical and theoretical value of making modality distinctions and working with pure traits—called 'conditional' factors—in psychometry, arises from the fact that everyday life frequently sets up the same conditions of constancy of either incentives or complexities, and situations in which incentives and complexities vary, or can be manipulated to vary, independently.

5. Further, novel, conditional modalities could be set up by extended application of the same principle. The most obviously useful are those obtainable by cleavage of the ability modality. Truncated, restricted factors of this kind are already implicitly recognized in personality predictions in special fields.

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(Manuscript received 1 June 1944)

PUBLICATIONS RECENTLY RECEIVED

Britain and her Birth-Rate. A Report prepared by MASS-OBSERVATION for the ADVERTISING SERVICE GUILD. London: John Murray. 1945. Pp. 245. 21s. net.

This is a most timely and suggestive study of a topic of the greatest social importance. All the conclusions which it reaches cannot be accepted without reserve; but it does nevertheless present striking and hitherto unobserved aspects of a problem which is fraught with difficulties. The data, based on answers to questions made in interviews, on written statements and on letters to a clinic and to the Radio doctor, showed first of all the very widespread confusion and ignorance which exist about the declining birth-rate and the probable decrease in population; and also that a considerable number of people think this decrease to be unimportant, or even desirable. The attitudes of people towards size of family and their motives for having children are of course extremely complex and confused, and it is sometimes a little difficult to follow the analysis of them which is given here. But in general it does emerge that few people nowadays really want to have more than three children at most; larger families are quite out of fashion. Now according to the calculations given here, the *average* number of children in a family must be at least three, to prevent a decline in population; and to attain this average, many women would have to have more than three children.

A useful distinction is made between the 'wanted unborn' and the 'unwanted unborn'. Among the former are those who are wanted but are not conceived, either from lack of fertility, or because the parents have definite motives for not conceiving them. Such motives include ill health, fear of childbirth (which still retains its terrors for many of those who cannot afford the best medical attention); inadequate housing, lack of domestic help and interests other than household drudgery; and economic insecurity—particularly the fear that extra children would mean a poorer chance in life for those already born. But if all these difficulties could be overcome and all the 'wanted' were born, the birth-rate would not be raised sufficiently according to the calculations from the data given here. The 'unwanted unborn' are needed too—those whom the parents do not at present wish to conceive in any circumstances; and here it is lack of faith in the future which seems to be the operative motive. It occurs particularly among the more educated, thoughtful and responsible members of the community. Whereas, the people who do have large families are those who care little how many children they have, nor how they are brought up, nor what happens to them. And legislation, though it might increase the number of births among this section of society, would have little effect upon the birth-rate of the more desirable sections.

If then the data and the reasoning of this book are to be accepted, this country is faced with the gloomy prospect that its population will decline at an increasingly and dangerously rapid rate, because "man cannot live by bread alone" but is unable to find something else to live by. This may be the case, but it is not yet proven. For instance, do conclusions drawn from people living in the south of England necessarily apply to those in the north who were not included in the sample interviewed? It seems quite likely that they do not. Is a low reproduction rate due everywhere to the same cause—for instance, in Sweden, with the lowest rate in Europe? Or must other equally fundamental causes be sought for? Admittedly evidence as to basic motives is very hard to obtain. It can be gained satisfactorily only by direct interviews; and even then the interpretation of the answers and the inferences to be drawn from them are more important than their manifest content. Thus it is unfortunately true that the above conclusions, so important and far-reaching, and so very possibly valid, cannot be accepted as they stand. More, and more reliable, evidence must be obtained from a larger number and wider range of people. Nevertheless Mass-Observation has done valuable service in adumbrating these aspects of the problem.

Psychology for the Armed Services. Edited by EDWIN G. BORING. Washington: The National Research Council. The Infantry Journal. 1945. Pp. xvii + 533. \$3.00.

This book is a development of *Psychology for the Armed Services*, the publication of which received an appreciative notice in the *British Journal of Psychology* in 1944. The same editor and the same impressive group of collaborators have here resumed all the material of the earlier book, and added much excellent discussion of basic principles. Written in the first place for the American armed services, the volume deserves the widest possible international circulation, and should take its place as a classic in its field. It may well be that readers in every other country will feel a little disappointed that the work of their own psychologists has received comparatively slight recognition. But this was inevitable, and the honest critic will have to admit that the annotated bibliographies appended to each chapter have been extraordinarily well chosen. As to the discussions themselves, they are clear, concise, they cover a very wide

field of topics and they will be of value in almost every branch of applied psychology, as well as in that of the fighting services. It is a thoroughly good and responsible production.

When this has been fully appreciated, it remains that a new book is already called for. Not only in the U.S.A. but in this and other countries, the late war brought psychology into new fields, presented psychologists with new problems, and developed many essentially new psychological methods of approach. As soon as possible these also must be collated and critically discussed and appraised.

Aviation Neuro-Psychiatry. By R. N. IRONSIDE and I. R. C. BATCHELOR. Edinburgh: E. & S. Livingstone, Ltd., 1945. Pp. viii + 167. 8s. 6d. net.

This book was written while the authors were on service overseas, and their aims were modest. It is put forward as a practical clinical guide and is not documented, no references being cited. It is not satisfactory, therefore, even as a short systematic treatise. Paradoxically, a second deficiency in the book arises from the authors' hope to counterbalance the omission of references by the "validity of personal experience". They overestimate the validity of the conclusions to which, by itself, clinical experience can lead and make statements open to serious criticism. For example, the only reference to aptitude tests in a chapter on "Selection for Flying" is the statement that they do not, "in our opinion, form a necessary part of the selection technique". Intelligence tests are also regarded as unnecessary in the majority of cases, since the "responses at interview will give a fair measure of intelligence". Reliance is placed on the reasons which candidates give as motives for joining aircrew.

Members of aircrew are exposed for periods which are relatively short to stresses which can be defined fairly clearly, and the development of their reactions to stress can be observed in greater detail than is usually possible in civilian practice. In this sense the causation of neurosis in aircrew is simplified and easily studied, and one might have hoped that principles would be evolved of the effects of stress on different types of personality. Some of the raw data are in this book, but it is disappointing that the only order brought into them is the conventional classification into affective disorder, hysteria and obsessional neurosis. The clinician is handicapped, however, for he treats individuals, and his obligations render it difficult for him so to plan his experience that it leads to new knowledge and sound generalization. The authors teach mainly from individual cases in the apprenticeship tradition of clinical teaching. No new principles emerge which would allow the practitioner to forgo experience, and, indeed, the authors state that experience can only be gained at first hand.

The section of the book dealing with the disorders themselves is the most pleasing for it contains brief notes of forty-four cases which have a special interest of their own. A section deals adequately with the clinical examination of aircrew patients. A section dealing with "Flying and the Normal Individual" is sketchy, and no mention is made of many of the recent contributions to the preservation of the mental health of aircrew.

D. R. D.

Forty-four Juvenile Thieves: Their Characters and Home Life. By JOHN BOWLBY. The International Journal of Psycho-analysis. 1944. Vol. xxv, Parts 1 to 4.

The characters and psychiatric histories of forty-four juvenile thieves have been compared with those of forty-four other children, all of whom had been referred to a Child Guidance Clinic. In sex, intelligence and economic status, in having psychotic or severely neurotic forebears and ambivalent and anxious mothers, there were no significant differences between the two groups. The one outstanding difference appeared when the separation for long periods in early life from the mother or mother-substitute was considered. Seventeen of the thieves had suffered complete and prolonged separation (for six months or more) during their first five years as compared with only two of the control group. To this separation is ascribed the development of the 'Affectionless Character' who is prone to truancy, stealing and prostitution.

These findings are entirely in accord with psycho-analytic theory. It would be interesting to compare them with data obtainable from other clinics.

H. B.

Social Concepts and the Child Mind. By HARRY ORDAN. New York: King's Crown Press (London: Humphrey Milford, Oxford University Press). 1945. Pp. ix + 130. 11s. 6d. net.

This monograph describes an experimental study of the apparent ability of children of various ages to understand 'social concepts', and of the relationship of this ability to intelligence and to social and economic status. This subject is one of the greatest interest and social importance. But the experimental

methods chosen were not such as to throw much light upon it. Children in two New York schools of different social and economic status were tested with: (a) a multiple choice test of knowledge of the categories into which should be put various terms connected with social and economic problems; (b) a multiple choice test of sentences illustrating the use of such terms; (c) choice between pairs of newspaper headlines as to which had the greater social significance. It is not perhaps surprising that the results correlated highly with intelligence as measured by Thorndike's vocabulary test; whereas, when intelligence was held constant, little or no correlation appeared with social and economic status. For it is clear that the first two tests related to knowledge of terminology, without necessitating any real understanding of any particular social problem. The third test, although it may have mirrored current social attitudes to some extent, was shown to be heavily weighted by the children's verbal familiarity with the terms used in the headlines. It is, however, interesting to note that the children showed more knowledge of terms connected with 'crime', than of those connected with the economic and political questions which they were supposed to have studied at school.

The Clinical Application of the Rorschach Test. By RUTH BOCHNER and FLORENCE HALPERN. 2nd, revised, edition. New York: Grune & Stratton. 1945. Pp. xi+331. \$4.00.

This is an excellent manual for the use of the Rorschach ink-blot test. It is clear and intelligible to the beginner as well as to the expert, and the former could not do better than use it in learning how to administer the test. Good records and interpretations are given for normal adults and children, as well as for abnormals—mental defectives, neurotics, behaviour problems, schizophrenics, alcoholics and organic cases. There is an extensive bibliography of Rorschach literature.

Rorschach's Test. II. A Variety of Personality Pictures. By SAMUEL J. BECK. New York: Grune & Stratton. 1945. Pp. xii+402. \$5.00.

The first volume of this book contained a description of the Rorschach procedure, and lists of responses. This volume continues with the interpretation of responses. A detailed discussion of the interpretation and its psychological significance, suitable for the Rorschach expert, is given first; and is followed by case histories to illustrate differences of intelligence, and studies of maladjusted adolescents, neurotic adults and schizophrenics. There are, however, no cases of normal children. A comprehensive bibliography is given.

Controlled Projection. By JOHN C. RAVEN. London: H. K. Lewis & Co., Ltd. 1944. With two test booklets. 12s. 6d. net.

This book is concerned with the presentation of various techniques of 'projection' testing. The main one used in the results described is to present the subject with a sketch of a man or woman, somewhat resembling him- or herself, and ask a series of questions about what this hypothetical individual likes, is interested in, is afraid of or worried about, etc. At the same time, the subject is asked to draw anything he likes. A similar technique can be used with children, and with groups of adults. It is considered that in his answers to the questions the subject will 'project' and develop his own ideas and desires.

A series of illustrative case histories and the corresponding projected material is given. But neither these cases nor the general results of the inquiry made by the techniques are discussed with sufficient clarity or detail to demonstrate how the projected material is used and interpreted. The answers may express the individual's conscious wishes and fears more clearly than would his answers to direct questioning, as at a psychiatric interview. But is it possible to use this technique to relate such wishes and fears to the personality as a whole, and to the individual's unconscious mental contents? Are the latter perhaps expressed in the drawings and 'doodlings'? If so, in what way?

The method and its results need a more systematic exposition before it is possible to judge their value.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY, 1945

GENERAL MEETINGS

- February 24, 1945. GENERAL MEETING IN LONDON.
 "Training Industrial Workers."
 (a) "Some Suggestions for the Development of Personality through Industry." By PEARL H. M. KING.
 (b) "Proficiency and Skill on the Job." By HELEN TURNER.
- April 5-10, 1945. ANNUAL GENERAL MEETING AT EXETER.
- April 5. **1st Session.** Business Meeting of the Society and Annual General Meetings of Medical, Education and Social Psychology Sections.
- April 6. **2nd Session:**
 "An Experimental Study of Suggestibility and Hypnosis." By W. D. FURNEAUX.
 "Surveys of Employee Attitude." By WINIFRED RAPHAEL.
3rd Session:
 "Psychological Aspects of Cooking for Oneself." By HILDE LEWINSKY.
 "The Reading Tastes of Adolescent Boys and their Bearing upon the Curricula of Continued Education." By W. D. WALL.
4th Session:
 "The Mosaic Test." By MARGARET LOWENFELD.
- April 7. **5th Session:**
 Symposium on Psychological Implications of the Concept of Culture-Patterns.
 Opened by T. H. PEAR, with papers by R. F. BARBOUR and J. C. FLUGEL.
6th Session:
 Election of Officers and Council, and election of Hon. Fellow, followed by Presidential Address:
 "Psychology in Medicine." By MILLAIS CULPIN.
7th Session:
 Display of Instructional Films arranged by the Visual Education Centre, University College, Exeter.
- April 8. **8th Session:**
 "The Devon Experiment in Education in Mental Health." By H. E. HOWARTH and JOYCE PARTRIDGE.
 "The Work of the Committee of Professional Psychologists (Mental Health) of the British Psychological Society." By J. M. GIBBS.
9th Session:
 "Perception and Understanding of Graphical Material." By M. D. VERNON.
 "Types of Problem Children." By CHARLES BURNS.
10th Session:
 "The Work of the Admiralty Psychologists." By ALEC RODGER.
 "The Problems and Methods of Visual Education." By G. PATRICK MEREDITH.
- April 9. **11th Session:**
 "Problems in the Occupational Adjustment of the Blind." By ERIC FARMER.
 "The Binet Test as Adapted for the Blind." By M. I. DUNSDON.
12th Session:
 Demonstration of Binet Tests for Blind Children. By I. W. LANGAN.
 "Some Differences between Boys and Girls in Vocational Guidance." By M. B. STOTT.

April 9.

13th Session:

"Refractory Period in Sensory-Motor Action." By K. J. W. CRAIK.

"Level of Aspiration as related to Neurosis and Temperament." By H. HIMMELWEIT.

14th Session:

Open Session for Discussion.

May 26, 1945.

GENERAL MEETING IN LONDON.

"Disorganization of Behaviour in Experimental Conflict." By D. RUSSELL DAVIS.

Demonstrations:

An Encephalogram Apparatus. By J. M. HARRISON.

Tests of Colour Blindness. By AGNES CRAWFORD.

September 22, 1945.

GENERAL MEETING IN LONDON.

"Statistical Psychology: a Plea for Scientific Method." By E. G. CHAMBERS.

"The Contribution of Psychology to Politics." By RANYARD WEST.

December 6, 1945.

GENERAL MEETING IN LONDON.

"Some Basic Considerations in the Dynamics of the Group." By W. R. BION.

"Some Sociatric Lessons from Officer Selection." By J. D. SUTHERLAND.

SECTIONAL MEETINGS

MEDICAL SECTION

April 5, 1945.

ANNUAL GENERAL MEETING AT EXETER.

November 21, 1945.

Conversazione and short addresses on the future policy and activities of the Section, summarized by H. V. DICKS.

December 19, 1945.

"Intra-Group Tensions in Therapy: their Study a Task of the Group." By W. R. BION.

EDUCATION SECTION

April 5, 1945.

ANNUAL GENERAL MEETING AT EXETER.

June 23, 1945.

"The Emergency Training of Teachers—the Goldsmiths' College Course." By M. M. LEWIS.

December 8, 1945.

"The T.A.T. A new Personality Test." By DOROTHY ARCHIBALD.

INDUSTRIAL SECTION

January 16, 1945.

"Psychological Problems of the Personnel Function in Industry." By A. H. SEYMOUR.

March 6, 1945.

"Industrial Assessments: Some Problems and Suggestions." By ALICE W. HEIM.

May 28, 1945.

"Industrial Training or Industrial Education?" By C. A. MACE.

July 3, 1945.

"Some Psychological Views on Salesmanship in the Retail Trade." By R. W. M. KRAUSS.

September 11, 1945.

"Merit Rating: Some Practical Considerations." By A. STEPHENSON.

November 6, 1945.

"Motion Study applied to Military Problems." By B. UNGERSON.

SOCIAL PSYCHOLOGY SECTION

April 5, 1945.

ANNUAL GENERAL MEETING AT EXETER.

May 4, 1945.

Informal Discussion Meeting.

June 1, 1945.

Informal Discussion Meeting.

July 3, 1945.

Joint Meeting with Royal Anthropological Institute:

"The Position of the Scapegoat in Modern Europe." By J. C. FLUGEL and MARGARET MURRAY.

September 29, 1945.

"The Psychological Significance of some Sociological Conceptions of the Group." By MARGARET PHILLIPS and HANS KELLER.

BRANCH MEETINGS

SCOTTISH BRANCH

- February 3, 1945. (Edinburgh.)
"Bilingual Children in Lewis." By CHRISTINA A. SMITH.
"Intelligence Testing in Fife." By D. M. McINTOSH.
- March 3, 1945. (Glasgow.)
"Psychiatric Observations in 90 Cases Treated by Prefrontal Leucotomy." By W. MAYER-GROSS.
"The Child-Parent Relationship and Frustration Types among Mentally Defective Juvenile Delinquents." By G. A. FOULDS.
- November 3, 1945. (Edinburgh.)
"A New Method of Teaching the Hard of Hearing." By LESLIE J. HEATH.
"Psychiatry on an Organic Basis." By ANDREW PATERSON.
- December 1, 1945. (Glasgow.)
"The Equilibrium Theory of Consciousness." By F. DE HAVAS.
"A Psychological Study of the Peptic Ulcer." By P. A. D. GARDNER.

MIDLAND BRANCH

- February 28, 1945. (Birmingham.)
"The Problem of Neurosis in Industry." By RUSSELL FRAZER.
- July 12, 1945. (Birmingham.)
"The Behaviour of Young Children who have experienced Air Raids and Evacuation." By E. M. JOHN.

NORTHERN BRANCH

1945. (No meetings.)

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AS AT 31 DECEMBER 1945

Revenue Account for the year ended 31 December 1945

1944		EXPENDITURE		INCOME	
£	s. d.	£	s. d.	£	s. d.
100	0	T ₀ Rent	...	By Subscriptions 1944	...
42	10	Rates	...	Subscriptions 1945	...
27	17	Lighting, Heating and Cleaning	...		...
622	5	Salaries, State Insurance and Staff- Annuity Fund	...	Less Resignations and Removals	...
145	13	Printing and Duplicating	...		...
		Printing copies of Memorandum and Articles	...	Transfer Fees:	...
10	11	Expenses of Meetings	...	Fellows	...
19	19	Travelling Expenses	...	Associates	...
94	7	Postages and Telephone	...		...
15	19	Stationery	...	Journal Subscriptions—Australian Branch	...
12	0	Insurance	...	Interest on Investments	...
12	12	Audit Fee 1945	...	Balance, being Excess of Expenditure over Income	...
7	12	Teas	...		...
2	2	Subscriptions to Outside Associations	...		...
12	8	Sundry Expenses	...		...
		<i>Journals, Publication Expenses and Reserves:</i>			
245	8	General	283 5 0		
115	5	Medical	118 17 6		
145	2	Educational	171 7 6		
505	16	Annual General Meeting 1945	573 10 0		
26	18	Scottish Branch Expenses	46 4 10		
9	11	Midland Branch Expenses	9 4 6		
3	1	Northern Branch Expenses	1 6 4		
5	17	Committee of Professional Psycho- logists (Mental Health)	32 7 2		
30	8	Bank Charges and Cheque Books	1 0 0		
1	16	Legal Expenses	5 5 0		
		<i>Depreciation:</i>			
		Library	49 1 9		
		Office Furniture and Fittings	18 0 0		
71	19	Surplus	67 1 9		
1781	9				
75	12				
1857	2				
£2227	16				

Balance Sheet as at 31 December 1945

1944			1945			1946		
£	s.	d.	£	s.	d.	£	s.	d.
LIABILITIES			ASSETS					
Staff Amnity Fund	...	...	Cash at Barclays Bank Ltd.	...	...	...	...	...
Sundry Creditors	...	...	Postage Stamps in hand	...	...	...	...	...
Subscriptions in Advance	...	...	<i>Investments at Cost:</i>	...	...	...	...	...
<i>Publications Reserve:</i>	...	...	£3514 9s. 6d. 3½ % Inscribed War Loan	3625	7	9	...	...
Balance as per last Balance Sheet	...	...	£1750 3% Defence Bonds	...	...	1750	0	0
Add Amount Reserved for Journals for year	...	...	£375 National Savings Certificates	...	...	375	0	0
	...	...	£125 3% Savings Bonds	...	...	125	0	0
	499	10 0						
	541	9 3						
	1040	19 3						
	5125	7 9						
	5875	7 9						
	5646	15 10						
	61	0						
	73	6 8						
	162	0 0						
	442	0 0						
	87	5 0						
	50	0 0						
	£6239 18 10							
	£6770 12 0							
	£6239 18 10							

Note: Market value of above Securities at 31 December 1945 £5880

Sundry Debtors
Subscriptions in Arrear
Office Furniture and Fittings:
Balance as per last Balance Sheet
Less Depreciation

Library:
Balance as per last Balance Sheet
Add Expenditure during year

Less Depreciation

Monograph Publications:
Balance as per last Balance Sheet
Less Amounts repaid during year

Shares in the British Journal of Educational Psychology Ltd., at Cost

Auditor's Report to the Members: I have audited the above Balance Sheet dated 31 December 1945, and have obtained all the information and explanations I have required. In my opinion the above Balance Sheet is properly drawn up so as to exhibit a true and correct view of the Society's affairs according to the best of my information and the explanations given to me and as shown by the Books of the Society.

T. BARRETT,
Incorporated Accountant,
Auditor

21 February 1946

S. J. F. PHILPOTT
Hon. Treasurer
GODFREY THOMSON
President